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An Address
ON THE
TREATMENT OF MOBILE BACKWARD DISPLACEMENT OF THE UTERUS.

DELIVERED TO THE NORTH OF ENGLAND OBSTETRICAL AND GYNAECOLOGICAL SOCIETY

BY

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THE chief aim of a society such as ours is the encouragement of research and the enlargement of knowledge in our special department. But I think it is advisable from time to time to review opinions which seem to have become more or less established and to subject them to criticism inspired by a healthy scepticism. It is for this reason that I am asking the members of the society to give me their individual opinions on the treatment of retroversion in general, and in particular on fixation and suspension operations in cases of mobile retroversion.

I need not conceal from you at the outset that it is a subject on which I hold very strong views, and that these are not the views of the majority of gynaecologists all over the world. But I believe that the general opinion is based on a false pathology, and that, apart from a few exceptional cases, these operations are unnecessary, and in many cases distinctly harmful. A good deal of what I have to say is not new, but was contained in a paper communicated by Dr. Fletcher Shaw and myself to the Royal Society of Medicine in 1910.

The Significance of Backward Displacement.

Before we can profitably discuss the question of treatment we are bound to give some attention to the problem that lies at the back of all treatment—namely, the problem of what is the real significance of backward displacement. There is no subject in gynaecology in which there is more confusion. We have only to refer to the accounts of the symptoms as described in the modern textbooks to find much divergence of view. It is many years since Matthews Duncan wrote in this connexion, "The subject is surrounded in a mist, and I do not myself know the straight and scientific road to the clean and sweet drops of truth." But he had one clear conviction which he lays down as a definite proposition: "a retroverted, a retroflected, or anteverted or anteflected, or a laterally displaced womb does not cause any pain or disorder or disability. Thousands of blooming, happy, fertile women have displacements." He admits that sometimes a retroversion is accompanied by symptoms, and under these he mentions "sacrache, lower lumbar ache, aching in the hips or in the groins or in the thighs, feelings of bearing-down, bladder trouble, and rectal trouble. . . . All of these and more, or none at all, may be present." He makes no mention of menorrhagia or leucorrhoea, or dysmenorrhoea.

The Beginning of the Theory of Passive Congestion.

Some years before Duncan wrote these lines, John Williams had been puzzled by the association of dysmenorrhoea and retroflexion. He seemed to have come to the conclusion that dysmenorrhoea was more commonly found when the uterus was retroflexed than when it was anteflexed, and he formed a theory to explain this. He decided that the pain was due to congestion and that the cause of congestion in retroflexion was the constriction of the body of the uterus and of the veins in the broad ligaments by the sacro-uterine ligaments. He had no "ocular" proof until "Dr. Squarey brought him a uterus which was slightly retroflexed. Posteriorly along the sides of the body of the uterus were distinctly seen the marks of these ligaments. The flexion was but slight and no parts of the broad ligaments had been drawn within the grasp of the constricting bands. Yet the marks of the bands on the body of the uterus unmistakably pointed out their constricting action." From this solitary *post-mortem* specimen sprang the congestion theory, to widen out in its application in the course of years. At first it was only used in connexion with dysmenorrhoea associated with retroflexion, but now it is also brought forward as an explanation of menorrhagia and leucorrhoea. In other words, congestion is the cause of disordered uterine function.

Herman adopted this theory with enthusiasm, and embodied it in his textbook (1907). He wrote: "If the body of the uterus sinks between the sharp and firm utero-sacral ligaments, the veins in the broad ligaments are pressed against the sharp lateral boundaries of Douglas's pouch, and the return of blood through them is hindered and congestion of the body of the uterus is the result." He thinks this occurs in about one case in ten, and one gathers that the other nine have no symptoms.

Present-day Views.

Now let me refer to some of the modern textbooks—those that are in common use by students and which are written by men of high standing, and whose opinions therefore carry great weight.

Jellett says that "the symptoms caused by backward displacement are in the main due to the endometritis by which the condition is almost invariably accompanied."

Bland-Sutton and Giles distinguish between the symptoms of simple and complicated retroversion. In the simple variety the patient complains of a feeling of ill defined weight and fullness in the pelvis. "When complicated by pelvic inflammation the symptoms are: pain, severe dysmenorrhoea, irregular metrorrhagia, and abundant leucorrhoea."

Blair Bell avoids a description of the symptoms of retroversion, but states that "if the retroflexion and version are of long standing a state of affairs may occur which leads to interference with the venous circulation, with oedema of the endometrium and giving rise to menorrhagia."

Fothergill discusses the question more fully than most, and is definitely of opinion that "the twisting of the parametrium is in some cases a prime cause of uterine congestion." But he states that "there is nothing more difficult than to diagnose the exact relation of the retroversion to the symptoms and the other physical signs present in any particular case." In favour of operations performed with the view of keeping the uterus in anteversion, he states that, "in some cases the uterus becomes retroverted when left to itself, and when retroverted becomes congested sufficiently to cause symptoms."

Eden and Lockyer are more sceptical. They remark that "it is open to doubt whether backward displacement has any influence upon the uterine circulation." They mention the possibility of the utero-sacral ligaments exercising pressure and causing a broad ligament varix, but think this is "probably very rare." "Menorrhagia and leucorrhoea are the result of accompanying endometritis."

In the manual written by ten London teachers a good deal of space is devoted to the effects of backward displacements on the endometrium and muscle coats.

> "There is no doubt," they say, "that the symptoms commonly complained of in acquired cases are the same as those of endometritis and uterine congestion generally," and therefore "it is necessary to demonstrate, if possible, how a retroverted uterus can develop endometritis or become congested."

There is interference with the venous return from the uterus. The fundus carries the broad ligaments with it and folds them over backwards. This involves some slight twisting of the vessels in the mesometrium, which tells on the veins only. The other view—"a very old" one, not so certain—is that the uterus is held down and incarcerated by the utero-sacral ligaments.

> "There can be no doubt that long-continued congestion will lead to thickening of the endometrium and degeneracy of the muscle coat with increase of the fibrous stroma, consequently the endometritis and the so-called metritis may develop with their usual symptoms. The symptoms are backache, bearing-down, menorrhagia, dysmenorrhoea, sterility, leucorrhoea, and chronic left-sided pain. Menorrhagia and dysmenorrhoea are the direct result of congestion of the endometrium. Leucorrhoea is the result of cervical catarrh."

Chipman states that "complications of retroversion in the first instance assume the form of a passive congestion

the result of the impeded return of the venous blood." He seems to think that "rotation and pressure against the utero-ischial ligaments combine to cause this result." The results of this passive congestion are the production of chronic oedema, with hypertrophy and hyperplasia, both in the mucosa and fibro-muscular wall of the uterus. Webster states both views: that endometritis, so called, is antecedent to the displacement, and that it is caused by venous congestion; but gives no positive opinion. He remarks that the venous network is so rich that it is difficult to understand how a marked obstruction can be caused.

It will be seen that there is a wide difference amongst these authorities as to the symptoms of retroversion. Duncan believes that retroversion causes nothing more than discomfort or aching in the pelvis and abdomen. Bland-Sutton and Jellett think that the symptoms of disordered uterine function only occur when retroversion is complicated by pelvic inflammation. Webster mentions them, but gives no positive opinion as to their relation to retroversion. These authors, with Eden and Lockyer, may all be classified as sceptical of the theory that retroversion can act as a cause of menorrhagia, dysmenorrhoea, and leucorrhoea. Herman, Fothergill, Chipman, and the ten London teachers are out-and-out supporters of the theory that retroversion may be the cause of disordered function, and explain at some length the method in which it acts. It is all a question of congestion. Herman believed that it was all due to a nipping of the uterine veins by the sharp edges of the utero-sacral ligaments; Fothergill puts it down to the rotation of the uterine veins; Chipman thinks it is produced by a combination of these two conditions; while the ten London teachers are evidently disposed to reject the explanation of Williams and Herman and to regard the troubles as due to rotation. It may be noted that none of these authors give any proof of the congestion theory. There are many difficulties in the way of its acceptance.

Objections to the Passive Congestion Theory.

1. If the uterine veins are sufficiently compressed to cause venous congestion, one would expect the whole of the uterus, including the cervix, to be of a blue colour. Has anyone ever seen this in a case of retroversion? If so, he has remained strangely silent. I have for years tried to find it in every case of retroversion—by inspection of the cervix in all cases, and of the uterus whenever an abdominal operation has given the opportunity—and have failed.

2. The arrangement of the circulation in the broad ligaments is such as to make it very difficult to compress the venous return from the uterus.

3. There is no evidence of congestion in cases of bad prolapse, except when a greatly hypertrophied cervix has been nipped at the vaginal orifice. In procidentia it is not found, and yet this is the case of all others in which the veins are stretched to an extreme limit, and in which we would expect the blood return to be greatly hampered.

4. In cases in which a retroverted uterus has been removed no dilatation of the veins can be shown on microscopic examination. If the congestion theory be correct, we ought to be able to distinguish by the microscope a case of chronic metritis (or something like it) produced by retroversion and one associated with anteversion. If long-continued congestion leads to degeneracy of the muscle coat with increase of the fibrous stroma, it will show quite a different picture from the ordinary chronic metritis, in which both muscle and fibrous tissue are increased.

5. If the haemorrhage is due to venous obstruction we should expect to have a venous type of oozing, but the menorrhagia from a retroverted uterus differs in no respect from that of an anteverted one.

6. Even if we have haemorrhage caused in this way, how do we account for the leucorrhoea? Leucorrhoea must mean increased activity of the glands. Does passive congestion lead to hypertrophy or hyperplasia of the glandular element of the endometrium? If so, how? There is a link missing in the chain of the congestion argument. There is mostly silence on this point with some general allusion to oedema. But Chipman says that chronic oedema produces hypertrophy and hyperplasia of the mucosa—in other words, that passive congestion causes overgrowth of normal tissues. The ten teachers are more cautious, and state that congestion leads to "thickening" of the endometrium. In another sentence they say that leucorrhoea is the result of cervical catarrh; but whether the cervical catarrh is due to venous congestion or is simply an incidental accompaniment of the retroversion they do not state.

7. The results of treatment form another objection to the theory, but these will be dealt with later.

The Relation of Disordered Uterine Function to Backward Displacement.

The symptoms of disordered function—namely, menorrhagia, leucorrhoea, and dysmenorrhoea—are the most common symptoms of chronic uterine disease. They are the symptoms—all or some of them—of what is called endometritis and chronic metritis, and occur more frequently in cases of anteversion than in cases of retroversion. Are we to give a different account of their origin according to the position of the uterus? Is it not more likely that they are produced in the same way, and that the position of the uterus has no say in their production? Further, is it not probable that the enlargement of the uterus, which is generally associated with endometritis and especially with chronic metritis, may be a principal predisposing cause of retroversion if the uterus is somewhat unstable in its equilibrium? In that case the sequence of events would be:

1. Endometritis or chronic metritis,* with disorders of function.
2. Enlargement of the uterus.
3. Backward displacement.

The congestion theory puts the sequence as:

1. Displacement of normal uterus.
2. Passive congestion.
3. Enlargement of uterus and disorders of function.

I have dealt at length with the question of the significance of retroversion, and to some it may seem to be of little practical importance. But the treatment adopted must follow the view taken of the pathology. If one believes that the *fons et origo* of all the symptoms mentioned is the backward displacement, then the logical result is to try and restore and keep the uterus in what is generally called a normal position. But a careful study of the evidence will, I think, force the conclusion that endometritis and metritis are not the results but rather the cause of—or at least merely complications of—retroversion.

There are other symptoms which have a better claim than those of disordered function to be regarded as produced by retroversion. I refer to the feeling of ill defined weight and fullness in the pelvis and of aching in the lower abdomen, especially in the iliac regions. These may be intensified (1) by the association of some degree of prolapse with the backward displacement, or (2) by increased weight of the uterus.

1. *Prolapse.*—It is not difficult to distinguish between simple retroversion and the retroversion which is found in the less severe cases of prolapse. In the former the cervix points more directly forwards and there is no descent of the upper part of the vagina; in the latter the cervix comes down in the axis of the pelvis and the body of the uterus is only moderately thrown back. The condition is primarily one of prolapse, and as such does not concern us in this discussion.

2. *Increased weight* may be due to hypertrophy of uterine tissues (endometrium or mesometrium) or to pregnancy or fibroid tumours. The two latter conditions are generally easily recognized. The enlargement which is found in chronic metritis may be considerable: it may be sufficient to cause incarceration of the uterus below the sacral promontory and so cause pressure on the pelvic veins (but not the uterine veins) or nerves, and even interfere with the functions of the bladder and rectum. It is in these cases that any rise in the intra-abdominal pressure, such as is caused by muscular effort or straining, may produce sudden acute symptoms of pelvic discomfort. Aching in the back is not a common feature except in cases of fixed retro-

* These terms are used simply as a matter of convenience. In the majority of cases it is probable that the changes are due to hypertrophy and have nothing to do with inflammation. I wish to emphasize this strongly, so as to avoid criticism that is concerned merely with words.

version, and then it is the result of old pelvic inflammation. This fixation of the uterus is another complication of retroversion which is outside of our discussion.

We may summarize the conclusions on the significance of retroversion we have so far arrived at as follows:

1. That complications are generally the only things that matter in retroversion, and not the displacement itself.
2. The complications are (*a*) endometritis and metritis; (*b*) prolapse; (*c*) increased weight; (*d*) adhesions.
3. None of these complications are produced by retroversion.
4. That in a certain number of cases a backward displacement seems to cause local pelvic discomfort and aching in the lower abdomen.

Varieties.

Some authors seem to lay great stress on the division of backward displacements into congenital and acquired—a division which seems to me of little practical importance. The so-called congenital forms are sometimes cases of malformation, and many of them are cases of maldevelopment. The most common form of maldevelopment is associated with an acute retroflexion, as the result of an area of weakness at the level of the internal os. This is more generally found in the acutely anteflexed or cochleate uterus, but occasionally it gives rise to acute retroflexion. The cervix is generally of an infantile type. The symptoms are practically the same in both classes and their consideration is omitted from this paper.

It is quite likely that some retroversions in unmarried women are due to some defect in the pelvic floor, but the retroversion in this case is merely an early stage of prolapse.

TREATMENT.

There is complete agreement amongst all gynaecologists that backward displacement may exist without any symptoms at all. It is often discovered quite by accident. It is generally agreed that these cases require no treatment. It has never been suggested that treatment should be undertaken in order to prevent the displacement from producing changes in the uterus.

Whenever treatment is required it is on account of complications, and by far the most common complication is what I have called "disordered function." In my experience fully 90 per cent. of the patients with retroversion who consult a doctor do so because they suffer from menorrhagia, metrorrhagia, leucorrhoea, or dysmenorrhoea. They may have only one of these symptoms, but not uncommonly they have them all. I regard these cases as cases of endometritis or chronic metritis. The retroversion is a matter of no importance except in a very small minority of cases. The treatment, therefore, is that of chronic metritis, and this resolves itself into curetting in the great majority. A few may be treated on palliative lines, and a few may seem to justify hysterectomy.

Curetting.

The results of this operation in cases of endometritis associated with retroversion are very good. In the paper which Dr. Shaw and I communicated to the Obstetric Section of the Royal Society of Medicine in 1910 our statistics showed that the results were even better than in cases in which the uterus was anteverted. In 75 cases of retroversion improvement followed in 88 per cent.; while in 267 cases of all kinds 86 per cent. were improved.

I have recently investigated a fresh series of cases of retroversion complicated by disordered function which were treated by curetting only. The numbers are smaller than in the former series, but the details are more fully recorded. On the patient's first visit the reasons why she sought advice, as well as her symptoms, were all recorded. No leading questions were put. These notes, together with the records of the operations and the answers to written inquiries, furnish the facts upon which my conclusions are based. Many cases had to be omitted in which something more than curetting had been done—such as excision of a glandular cervix, or a plastic operation on the posterior vaginal wall.

The results are as follows: Thirty-eight cases were investigated, but of these 5 are incomplete, as no replies were received in answer to the written inquiries. Of the 33 completed cases 28 reported themselves as improved, while 5 stated that there was no improvement.

When the 5 failures were investigated the results were as follows: The first had another operation—nature not stated—a year after the curetting. The second was no better six months after operation; she had a pessary inserted then, became pregnant and had a miscarriage, and has had two children since. The third became pregnant eight months after operation and had a living child—her previous child having been born four and a half years before the operation. The fourth became pregnant five months after operation, having been sterile before the operation. The fifth became pregnant nineteen months after the operation, having been sterile up to that time and having suffered from dyspareunia.

Thus, in the 5 cases reported as failures, 4 have had pregnancies after curetting (one after the introduction of a pessary) and one has had another operation. If we eliminate the 3 cases in which pregnancy occurred without other treatment, it brings out the figures as 30 patients with 28 cases in which improvement is notified—a percentage of over 93. Of the 28 who were improved, 11 say that they are very much better and are satisfied with the result; 14 merely state that their condition is better; and 3 say they are slightly better. These cases are dealt with more fully in an appendix. I might point out that the mere report of the patient as to her improvement was not taken as sufficient; particulars were obtained as to each individual symptom before and after the treatment.

The treatment by curetting of a uterus which has the physical characteristics and the attendant symptoms of chronic endometritis seems such an obvious proceeding that one is at a loss to account for the fact that this treatment is not even mentioned in connexion with retroversion by some of the authors from whose textbooks I have already quoted, and by others it is given quite a subordinate place as a means of cure. Possibly the authors may take this treatment for granted, but the only inference one can draw from their silence is that they do not make use of it in cases of retroversion; that, after all, the displacement is the thing that matters to them and not the complications. Their rule seems to be to treat a case of chronic endometritis by curetting if the uterus is anteverted, but by pessaries or suspension if it is retroverted. In the light of my personal experience—and I think I may say of the experience of my colleagues—curetting is at least of as much value in one set of cases as in the other. There is this to be said for curetting: it is a safe operation in the class of cases under discussion, with reasonable precautions. The only trouble is that when it is done inefficiently, as I fear is not uncommonly the case, it may prove to be useless.

MECHANICAL TREATMENT.

1. *By Pessaries.*

In older textbooks the treatment by pessaries is the only treatment described, and many pages are still devoted to the methods by which displacements are corrected and pessaries are introduced. It was the main resource of the gynaecologist forty years ago, and is still very popular amongst practitioners of to-day. There is no doubt that in some few cases benefit follows the insertion of a pessary, but I am convinced that in most it is merely a faith cure, or cure by suggestion; otherwise the relief obtained would often seem inexplicable. It is sometimes found that a patient is going about quite happily with a small pessary which is much too short to exercise any pressure on the posterior fornix or any tension on the cervix. Sometimes one finds that the instrument has been inserted so that the upper bar lies in front of the cervix. Even more commonly one finds that an instrument of suitable size has been chosen and is in correct position, but the uterus is still retroverted and hangs backwards over the top of the pessary. One even finds cases in which the patient is quite happy in

the thought that a pessary is doing good after it has been removed for some time. To quote Matthews Duncan:

"It is not a simple matter to judge of the part played by a pessary in relieving or removing painful symptoms. You will find many patients, alarmed at the idea of having a displacement and believing the pessary undoes it or cures it, wear an instrument with satisfaction and even pleasure, although it gives them new pains or increases what they had before. Such patients live in the pleasing and sustaining delusion that the pessary is curing them and object to its removal, even when removal gives relief, although told that the pessary, when in, does not alter the condition of displacement."

But allowing for all this, there are undoubted cases in which the patient feels a real relief. Many of these are cases of prolapse with accompanying retroversion, and in these we can understand that the bearing-down sensation is relieved. There is, however, a residuum of cases in which there is no sign of prolapse, but the bimanual replacement of the uterus and its retention in a forward position do produce quite a remarkable improvement. What is the explanation? Why is it that the pressure of the body of the uterus in the pouch of Douglas should cause discomfort in some cases? It is not a question of increased weight or bulk, because we sometimes find it absent in cases of heavy retroversion and present in retroversion of an otherwise normal uterus. In a small proportion of cases the relief caused by pushing the uterus forward is undoubted. It seems to me to be due to some effect on the sympathetic nervous system. In exceptional cases a retroversion seems to be related to a neurasthenic condition. In these cases the patient complains not only of local discomfort, but of aching in the lumbar and dorsal spine, and even of occipital headache, all of which are removed or relieved by pessary treatment. This improvement is entirely a question of comfort. I have never heard or known of a case in which the insertion of a pessary has banished menorrhagia, leucorrhoea, or dysmenorrhoea, when these have been present before the instrument was employed.

This seems to be a suitable place for a few notes on the subject of retroversion and pregnancy. I think most of us hold the view that retroversion acts sometimes as a bar to pregnancy. We have all been consulted by young women who have been married for some months or years and have come to ascertain why there has been no pregnancy. An examination has revealed a retroversion. The introduction of a Hodge pessary after bimanual reposition seems in many of these cases to produce the desired effect. I think that we are also agreed as to retroversion being a predisposing cause of abortion. How it acts we do not know, but we are justified, in cases of early pregnancy in a retroverted uterus, in making use of a pessary for a time.

There is no doubt in my mind that pessaries are not nearly so commonly used as they were thirty years ago. The objections to their use are obvious. They must be objectionable to most patients with a properly developed sense of cleanliness, and frequent cleansing of a Hodge pessary means frequent visits to the doctor.

The cases in which pessary treatment, except as a temporary measure, is of real use form a very small minority, and in these it is better that the patient should have recourse to surgical methods, except in the rare cases in which operation is contraindicated by the general condition of the patient.

2. *By Suspension.*

It is not my intention to review all the various operations that have been devised with the object of retaining the uterus in a position of anteversion; their name is legion, and the amount of thought expended in devising satisfactory methods has been enormous. The literature must consist of many thousands of printed pages, and the number of these operations performed daily in all countries must be very large. Alexander's operation, ventro-fixation, ventro-suspension, Gilliam's operation and its modifications, and Webster's sling operation, all have their advocates, and each attaches much importance to some particular detail of technique. From the large number of cases reported, I am driven to the conclusion that to many operators the sole requirement for the operation is that there should be a backward displacement. Many, no doubt, believe that when the uterus is elevated by pessary or stitches the symptoms of disordered function will disappear. There is not a vestige of proof for this statement. If it were true the same treatment should be employed for heavy anteversion.

At a meeting of the British Medical Association some years ago one speaker said he was tired of arguments on this subject, and felt inclined to exclaim, "A plague on both your houses." In cases of retroversion he curetted first and then suspended the uterus. He evidently did not share in the sentiment of the Latin poet and philosopher who wrote "*Felix qui potuit rerum cognoscere causas*," and it did not occur to him that in many cases he might be subjecting his patient to unnecessary pain at the time, and to possible danger in the future. For, after all, an abdominal section should not be lightly undertaken. There is always a risk of intestinal obstruction after these operations, although the risk is no doubt small in the hands of experienced operators. But if every case were published in which there had been trouble or disaster as a result of these operations, I believe the result would astonish most of us. It is a pity that many of the operators cannot see the results of their handiwork some years later—the long thin bands that are the only evidence of a previous ventro-suspension, the intestinal adhesions, and the weak abdominal walls. Within the last few years I have operated on at least ten cases with the object of freeing adhesions, dividing bands, and attempting to cure ventral herniae—all as the result of previous suspension operations.

But, after all, it may be asked, Why, if these operations are not satisfactory in every way, are they performed with such frequency? Surely, it may be argued, the results must be good. The explanation seems to me to be threefold:

(1) The operations are done in such large numbers—many operators counting their cases by the hundred—that the only indication for the operation in many of the cases must be the displacement alone. It follows, therefore, that in these circumstances the patients are naturally none the worse for the operation unless things go wrong.

(2) Once any operation becomes the fashion, the critical faculty of the individual surgeon is apt to be suspended. The history of medicine gives many striking examples of this. In gynaecology we can remember the extraordinary vogue of pessaries and of tampons and swabbing with lotions in the treatment of out-patients; and of the removal of ovaries which we now know must have been more or less healthy. In general surgery we have heard of the extraordinary and far-reaching benefits which have been claimed for fixing the kidneys or for dividing intestinal kinks.

(3) The power of suggestion is great. I think the cure by faith and by suggestion is just as common after suspension operations as it is, or was, in pessary treatment. All women are upset when they are told they have a displacement, and naturally relieved when they are assured that the uterus is now in the right position. It may be objected that the element of suggestion may account for much of the success of curetting, but here it cannot come so powerfully into play. The displacement is still there after the operation, and no attempt has been made to reduce it.

I would ask those who are firm believers in suspension operations to tabulate the several indications for each operation. We ought to know in every case why it was done—what were the symptoms that seemed to call for it. Was it on account of backache or bearing-down sensations or fullness in the pelvis, or interference with the functions of the bladder; or was it because the patient suffered from dysmenorrhoea or menorrhagia or leucorrhoea or dyspareunia or sterility or frequent abortions? In the subsequent history we should be informed as to the effect of the operation on each individual symptom.

I have now to admit that there are some few cases in which a suspension operation has to be done. The reasons are obscure, but the cases which are suitable are just those to which I referred in connexion with the use of pessaries. There are some cases in which all complications are got rid of and yet the patient finds that the retroversion is intolerable. I believe these cases are very few. In my own practice I have performed four ventro-fixations

in the last four years; and in all of these cases curetting had been tried in the first place.

I have little to say in regard to the different kinds of operation. My own preference is for ventro-suspension or suture of the anterior uterine wall—at a point just above the os externum—to the anterior abdominal wall without removal of its peritoneum. Actual fixation or fixing of the uterus to fascia seems to have quite gone out of fashion, except, perhaps, for prolapse. But the other varieties of operation all have their advocates. There is one that seems to me a striking example of misplaced ingenuity, and that is the operation known as "Webster's sling." It seems difficult to understand why men who hold the belief that the uterus may become the victim of congestion through a comparatively slight change of position should advocate an operation the result of which is to tie up the uterus in its own round ligaments. I think if the object were to try and produce a condition of passive uterine congestion by means of an operation that this method might be favourably considered.

My remarks have been concerned with the treatment of mobile retroversion. The application of suspension operations to prolapse, with or without retroversion, and to cases in which the uterus is adherent, do not come under discussion.

Summary.

My views on the treatment of mobile backward displacement of the uterus may be summed up as follows:

1. The great majority of cases of retroversion in which treatment is required are cases in which the symptoms are mainly those of disturbed function. The treatment, therefore, is the same as for cases of chronic endometritis—namely, curetting. In a few cases where there is advanced chronic metritis hysterectomy is justifiable.

2. Pessary treatment is indicated as a temporary measure in some cases of sterility and of early pregnancy associated with retroversion. It may also be indicated in a small proportion of cases in which the backward displacement gives rise to discomfort, provided that for some reason the case is not suitable for operation or the patient refuses.

3. Suspension operations are no cure for symptoms of chronic endometritis; when these are present curetting is indicated.

4. In a small proportion of cases—probably not 10 per cent.—when the patient complains of pelvic discomfort or pain a suspension operation may be necessary; but only after curetting has been given a good trial.

Brief Notes of Cases.

The following is a list of questions which was sent to each patient:

1. Are your periods regular?
2. Do you lose less at the periods than before the operation?
3. Have you any pain at the periods?
4. Have you any aching in the lower part of the abdomen between the periods?
5. Have you any discharge (white or coloured) between the periods?
6. What in your opinion is the result of your operation: Is your condition better? Is your condition unchanged? Is your condition worse? If worse, please give particulars of your symptoms.

Case 1.—Nullipara, aged 31; married two years. Admitted to St. Mary's Hospital, Manchester, on account of dysmenorrhoea, leucorrhoea, and sterility. Operation, February, 1920. *Result:* "Distinctly better." Still some dysmenorrhoea; no leucorrhoea. Has had three children since operation.

Case 2.—Single woman, aged 23. Suffered from menorrhagia, leucorrhoea, and dysmenorrhoea. Operation at St. Mary's Hospital, April, 1920. *Result:* "Better." No menorrhagia; still some leucorrhoea and dysmenorrhoea. Married since operation.

Case 3.—Married woman, aged 29; two children; last confinement two years previously. Suffered from menorrhagia, leucorrhoea, and chronic abdominal pain. Operation, September, 1923, at nursing home. *Result:* "Better." No menorrhagia or leucorrhoea; still slight abdominal pain.

Case 4.—Nullipara, aged 26; married two years. Operation, December, 1921, at St. Mary's Hospital, on account of menorrhagia, leucorrhoea, chronic abdominal pain, and dyspareunia. *Result:* "Better." Still menorrhagia and some leucorrhoea; no abdominal pain; no mention of dyspareunia. Improvement probably refers to dyspareunia and abdominal pain.

Case 5.—Married woman, aged 34; three children, youngest aged 5. Admitted to nursing home with profuse and irregular haemorrhage, and operated on in May, 1923. *Result:* "Great success." No menorrhagia.

Case 6.—Single woman, aged 21. Suffered from dysmenorrhoea, menorrhagia, and leucorrhoea, for which she underwent operation at a nursing home in February, 1922. *Result:* "Better." Less dysmenorrhoea, no menorrhagia, slight leucorrhoea.

Case 7.—Married woman, aged 31; one child aged 3. Admitted to St. Mary's Hospital on account of menorrhagia, dysmenorrhoea, leucorrhoea, and chronic abdominal pain. Operation, November, 1921. *Result:* "Better." No menorrhagia, very slight dysmenorrhoea, still leucorrhoea and some abdominal pain.

Case 8.—Married woman, aged 33; two children, youngest aged 6. Operated on in July, 1923, at St. Mary's Hospital, on account of menorrhagia and chronic abdominal pain. *Result:* "Better." No menorrhagia or abdominal pain.

Case 9.—Nullipara, aged 24; married nine months. Had acute dysmenorrhoea and chronic abdominal pain, for which she underwent operation at St. Mary's Hospital in August, 1921. *Result:* "Successful." Less dysmenorrhoea, no abdominal pain.

Case 10.—Nullipara, aged 34; married three years. Admitted to St. Mary's Hospital with menorrhagia and leucorrhoea. Operation, March, 1923. *Result:* "Slightly better." No menorrhagia, slight leucorrhoea. Small masses in pouch of Douglas (? adenomyomata).

Case 11.—Single woman, aged 26. Admitted to St. Mary's Hospital with dysmenorrhoea and leucorrhoea. Operation, April, 1921. *Result:* "Better." Less dysmenorrhoea, no leucorrhoea.

Case 12.—Nullipara, aged 36; married eight years. Admitted to St. Mary's Hospital with menorrhagia, and operated on in October, 1922. *Result:* "Much better." No menorrhagia. This patient was marked before operation as possibly suitable for ventro-suspension.

Case 13.—Single woman, aged 31. Suffered from dysmenorrhoea, leucorrhoea, and chronic abdominal pain. Operation at nursing home, July, 1922. *Result:* "Better." Less dysmenorrhoea, still leucorrhoea; some abdominal pain for five or six days. Cavity of uterus irregular (? small fibroids + 1½ inch).

Case 14.—Married woman, aged 35; two children, youngest aged 2. Operation at nursing home in June, 1922, on account of leucorrhoea and menorrhagia. *Result:* "Much better." Very little leucorrhoea, no menorrhagia.

Case 15.—Married woman, aged 31; nullipara. Admitted to St. Mary's Hospital with leucorrhoea and dysmenorrhoea, and operated on in April, 1922. *Result:* "Better." No leucorrhoea, still dysmenorrhoea. Before operation she was marked as possibly suitable for ventro-suspension.

Case 16.—Married woman, aged 28; one child, aged 8½ years. Operation at St. Mary's Hospital in July, 1922, on account of leucorrhoea and dysmenorrhoea. *Result:* "Better." Only occasional leucorrhoea, less dysmenorrhoea.

Case 17.—Single woman, aged 23. Suffered from acute dysmenorrhoea, leucorrhoea, and chronic abdominal pain. Operation at St. Mary's Hospital, May, 1923. *Result:* "Very beneficial." Very little pain at periods; no leucorrhoea, no abdominal pain. Patient had well marked cervical catarrh before operation.

Case 18.—Nullipara, aged 32; married two and a half years. Operation at St. Mary's Hospital, in February, 1921, on account of menorrhagia, leucorrhoea, and chronic abdominal pain. *Result:* "Slightly better." Symptoms still persist, but presumably less. There has been no pregnancy.

Case 19.—Nullipara, aged 32; married five years. Operation at St. Mary's Hospital, in March, 1923, on account of menorrhagia, dysmenorrhoea, and leucorrhoea. *Result:* "Better." No menorrhagia, still some dysmenorrhoea, very occasional leucorrhoea. Patient disappointed that there has been no pregnancy.

Case 20.—Nullipara, aged 26; married fifteen months. Suffered from menorrhagia and leucorrhoea. Operation at nursing home in January, 1921. *Result:* "Altogether satisfactory." No menorrhagia or leucorrhoea. Two children since operation.

Case 21.—Single woman, aged 24. Operation in nursing home, January, 1923, on account of dysmenorrhoea, leucorrhoea, and menorrhagia. *Result:* "Very much better." Very little pain, no menorrhagia or leucorrhoea. Patient had well marked cervical catarrh before operation.

Case 22.—Single woman, aged 22. Suffered from menorrhagia and chronic abdominal pain. Operation at St. Mary's Hospital in June, 1923. *Result:* "Considerably better." No menorrhagia or abdominal pain. Patient, at her first visit, was marked as possibly having an adherent uterus.

Case 23.—Single woman, aged 19. Operation in nursing home, July, 1923, because of dysmenorrhoea and leucorrhoea. *Result:* "Slightly better." Still dysmenorrhoea and leucorrhoea.

Case 24.—Single woman, aged 31. Had pain in right groin and hip, menorrhagia, and dysmenorrhoea. Operation at St. Mary's Hospital, March, 1922. *Result:* "Better." Now very seldom has abdominal pain; periods less; still dysmenorrhoea.

Case 25.—Married woman, aged 36; two children, youngest aged 3. Suffered from menorrhagia, leucorrhoea, dysmenorrhoea, and chronic abdominal pain. Operation at St. Mary's Hospital, February, 1924. *Result:* "Better." No menorrhagia, very slight leucorrhoea, slight dysmenorrhoea; still some aching in abdomen. Marked before operation as possibly suitable for ventro-suspension.

Case 26.—Married woman, aged 23; one child, aged 5 years. Suffered from chronic abdominal pain. Operation in July, 1923, at St. Mary's Hospital. *Result:* "Conditions perfect." No abdominal pain or menorrhagia.

Case 27.—Married woman, aged 40; two children, youngest aged 2 months. Operation in nursing home, March, 1921, on account of menorrhagia and chronic abdominal pain. *Result:* "Very much better for two years." No menorrhagia, abdominal pain less. Now has symptoms of climacteric.

Case 28.—Nullipara, aged 32; married nine years. Operation in nursing home, February, 1922, on account of dysmenorrhoea and leucorrhoea. *Result:* "Much better." Only occasional pain, no leucorrhoea. Pedunculate adenoma removed from uterine cavity by curette.

Case 29.—Married woman, aged 30; four children, youngest aged 18 months. Operation in nursing home, March, 1921, on account of menorrhagia and leucorrhoea. The result was not stated. Patient had to undergo another operation (details not given) in June, 1922. The only pain now is at the bottom of the back.

Case 30.—Woman, aged 24; married six months. Suffered from dysmenorrhoea and leucorrhoea, for which she underwent operation in a nursing home in December, 1920. *Result:* "No change." The reply refers to six months after operation. A pessary was then introduced, and pregnancy began six weeks later. Since then she has had one miscarriage and two confinements.

Case 31.—Married woman, aged 30; one child, aged 4½ years. Operation at St. Mary's Hospital in April, 1920, on account of menorrhagia, dysmenorrhoea, and leucorrhoea. *Result:* "No better." Still menorrhagia and leucorrhoea, not much dysmenorrhoea. Patient had another confinement two years after operation, and has been in bed for two months with "nervous prostration."

Case 32.—Nullipara, aged 19; married nine months. Was operated on for dysmenorrhoea, at St. Mary's Hospital in March, 1923. *Result:* "Unchanged." Still dysmenorrhoea and leucorrhoea. Patient was confined six months ago (March, 1924); she was pregnant three months after operation.

Case 33.—Woman, aged 30; married four months. Suffered from dysmenorrhoea, dyspareunia, and leucorrhoea, for which she underwent operation at St. Mary's Hospital in January, 1921. *Result:* "Unchanged." Still dysmenorrhoea and leucorrhoea. She was confined four months ago.

THE OPERATIVE TREATMENT OF KNEE FLEXION IN POLIOMYELITIS.*

BY

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I MUST apologize for bringing up a subject on which, to all intents and purposes, the last word has been spoken; but it so happens that within the last three years I have had fourteen cases under my care, in which the usual methods of continuous traction and wedge casts would appear to be insufficient, unless very protracted periods of time had been expended in such treatment.

With the limited number of hospital beds at my disposal and my very long waiting list, I felt that a quick method of deformity reduction was justifiable, and my experience has shown that such risks are negligible and that the deformity even in extreme cases is readily reduced within two or three days without circulatory interference. I may here add that, whatever vasomotor disturbances existed before operation, these were in no instance intensified. On looking up the standard works on orthopaedics I cannot find any definite instructions as to operative procedure: by one author we are advised to divide the hamstrings and by another the tight structures, but as far as I can judge no standardized operation has been formulated.

At the Shropshire Orthopaedic Hospital I am told that at least an average of three months is required to straighten out bad cases, and even then subsequent operation may be required. It is obvious that all preventable deformities which follow poliomyelitis will become fewer and of less severity as orthopaedic after-care schemes become more general, but for the present I am afraid we shall meet with them from time to time.

In the cases under discussion three were bilateral and eleven unilateral; all displayed an extreme degree of paralysis, muscular power, if present, being represented solely by the ileo-psoas or the tensor fasciae femoris. None of the bilateral cases had attempted to walk since the onset of the disease, and two of the three cases had never walked. All the unilateral cases were "dangle legs," and walked either with two crutches or with one crutch on the affected side round which the withered and useless limb was crooked. Where the triple deformity of hip flexion, knee flexion, and talipes equino-varus existed, as in most of the cases, the knee condition was dealt with first; this was followed by a Soutter's operation where required, and finally reduction of the foot deformity with stabilization finished the operative sequence. The youngest case was 10 years old, with onset at a few months, and the oldest 41 years, with onset at the age of 7. Two other adults were over 35—one paralysed at 12 months, and another at 6 years. Twelve cases were of pure flexion; it is interesting to note that the degree of posterior tibial luxation is slight or absent in all paralytic cases unless one of the hamstrings escapes. Two cases had marked posterior luxation and external rotation of the tibia in consequence of the biceps alone retaining function.

The degree of flexion varied from 30 to 100 degrees, all measured during passive extension of the joint. The average deformity lay between 45 and 60 degrees. The longest paralytic history was thirty-six years; he had 100 degrees of flexion with marked luxation and external tibial rotation. In this case by open operation the deformity was very markedly reduced and I was able to perform an arthrodesis of the knee simultaneously without any undue removal of bone. This procedure, combined with foot stabilization, enabled him to walk very well with nothing but a surgical boot with 4 in. elevation. The shortest paralytic history was six years. In this case I am prepared to admit that with prolonged stretching an open operation might have been avoided, but where paralysis is so extensive that future tendon transplantation is out of the question, one is guided solely by the principle of surgical democracy. As is well known, forcible extension of a flexed knee by manipulation may be productive of a tibial luxation where this has not previously existed.

Operation.

The patient is turned on his face and a sandbag is placed beneath the lower third of the femur; an assistant holds the foot and steadies the limb, and produces extension of the knee when called upon to do so; no tourniquet is used. A vertical incision in the middle line of the posterior aspect of thigh and leg is carried from the mid-point of the thigh to the junction of the upper and middle thirds of the leg. The deep fascia is incised and is found to have undergone adaptive shortening, the skin is widely undercut, and a transverse incision is made through the fascia about the joint level. The muscles bounding the popliteal space are dissected from the large mass of fat which surrounds the popliteal vessels and nerves and which is found to extend down to the popliteal fascia and the popliteal surface of the femur. This mass of fat is very much more pronounced in these cases than in normal limbs. Where hamstring tendons are tight (11 cases) all should be divided, care being taken that the external popliteal nerve is freed from the inner surface of the biceps muscle at the point of section of the latter. The vessels and nerves are freed in the fat mass from the inner hamstrings and the inner head of the gastrocnemius; this can be done by sharp or blunt dissection, but I have found that the easiest method of separation is by means of gauze on the glove finger. Muscular branches to the inner head of the gastrocnemius and the superior and

* Read before the British Orthopaedic Association, May 16th, 1924.

inferior internal articular arteries will require ligature. The inner head of the gastrocnemius is isolated, divided at its origin, and the muscle turned down. The fatty mass is then pushed to the outer side until the azygos vessels are seen perforating the joint capsule; these are divided between forceps and ligatured, and it is then found that by retraction and further dissection the entire mass, which includes vessels and nerves, is freed from the posterior aspect of the joint which is now in full view. In two cases there was no shortening of the hamstrings at all; in fact it was difficult to differentiate the muscles on account of the fatty degeneration, and section of the muscles was solely undertaken to get a better view of the ligament of Winslow. Transverse section of the capsule is accompanied by forcible extension of the leg by the assistant; it is found that no very definite extension is gained until the posterior ligament is fully divided from side to side; this section includes the short external lateral ligament and the tendon of the popliteus. I have not yet satisfied myself as to whether the popliteus is ever a deforming factor, as I have never seen the flexion deformity combined with internal rotation of the leg. The posterior articular surfaces of the femoral condyles now appear in the capsular wound, which becomes elliptical in shape as the result of extension of the leg. The posterior crucial ligament is now seen as a tight band which prevents full extension of the joint; on section of this ligament the limb easily extends, often by its own weight. It is sometimes necessary to dissect out the popliteal nerves from the fat mass to enable them to take a more superficial and direct course. The popliteal artery rarely appears tense, circulation in the foot generally returning within a few minutes. In only one case have we had to delay full extension for a day or two; here the last 20 degrees was obtained in this space of time by the use of gradual extension on a bed splint. The case is nursed for a fortnight in a bed splint, and when the sutures are removed the limb is encased in plaster; the hip and foot deformities are subsequently treated as has been stated. The shortening varies from 1 in. to 4 in., and the patient is taught to walk with a high boot and calliper. Passive movements of the knee are encouraged in all cases, so that in adults a jointed calliper can be substituted later. In one case manipulation under an anaesthetic was required to produce knee flexion beyond 60 degrees.

The operation is one of simplicity, and the only points that I wish to emphasize are:

(*a*) Section of the inner head of the gastrocnemius and the mobilization of the vessels and nerves to the outer side of the joint. The weakening of the gastrocnemius does not appear to have any detrimental effect later and no calcaneus deformity has arisen in connexion with foot stabilization.

(*b*) The division of the whole of the posterior ligament from side to side, and more particularly the importance of section of the posterior crucial ligament, which I regard as a prime deforming factor.

Table of Cases.

Case.	Initials.	Sex.	Age.	Limbs Affected and Duration.
1	G. H.	M.	10	Both legs useless from birth.
2	K. H.	F.	10	Both legs useless from birth.
3	J.	F.	35	Left; fifteen months.
4	F.	F.	24	Right; onset at age of 4 years.
5	H.	F.	36	Right; six years.
6	A. H.	M.	41	Right; onset at age of 7 years.
7	C. W.	M.	14	Right; two years.
8	E. J. M.	M.	12	Right; onset at age of 7 years.
9	H. W.	M.	12	Left; one year.
10	H. M.	M.	15	Right; onset at age of 3½ years.
11	G. M.	F.	14	Right; onset at age of 3 years. (In hospital.)
12	H. M.	F.	14	Both; never walked right. (In hospital.)
13	R. W.	F.	19	Right; onset at age of 2½ years. (In hospital.)
14	M. D.	F.	22	Left; onset at age of 15 months.

THE THEORETICAL FOOD REQUIREMENTS OF INFANTS.

BY

G. B. FLEMING, B.A., M.D.

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THERE seems to be a great diversity of opinion regarding the food requirements of infants during the first year of life. This is probably due to the fact that during this period the infant can only give expression to its needs in a very inadequate manner. There can be no doubt that many infants are underfed, while others are greatly overfed. Overfeeding is, however, probably less common than underfeeding, because usually unmistakable signs of gastro-intestinal disturbance are displayed before overfeeding has been persisted in for any length of time. With underfeeding, on the other hand, the signs are less striking and appear more gradually.

Most of the textbooks on pediatrics give instructions regarding the nature and composition of the food that should be given, but there is a very striking variation in the quantity recommended by different authors. For example, Still[4] advises that at 3 months of age an infant should be fed six times a day with a milk mixture containing milk 1½ oz., cream (48 per cent.) 1 drachm, and sugar 1 drachm, such a diet being equal to about 400 calories a day. Hutchison[8] would give a child of the same age

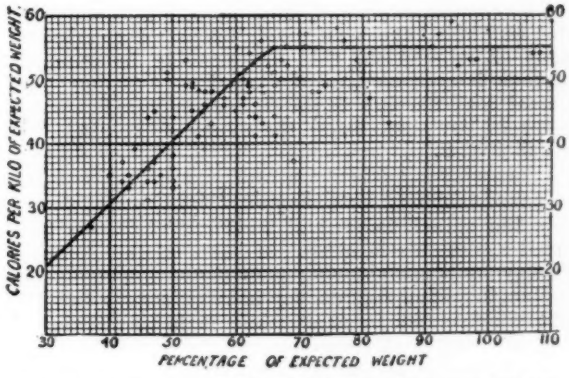

FIG. 1.—The basal metabolism of infants under 1 year per kilo of expected weight and degree of emaciation.

seven feeds consisting of milk 1½ oz. and sugar 1 drachm, equal to 300 calories a day; while Thomson[13] recommends milk 4½ oz. seven times a day, equal to about 600 calories a day, and the figure suggested by Holt[7] is 560 calories a day. Thus Hutchison would give a diet with only half the number of calories that Thomson suggests, while Still takes an intermediate position. Lusk[9] justly says: "The so-called 'scientific feeding' of infants is unworthy of the name unless the caloric requirement is carefully considered. From lack of this knowledge babies are frequently systematically starved." For this reason and from the fact that there seem to be such divergent views on the subject, an attempt has been made, by utilizing the results of metabolic experiments, to indicate the ideal requirements of infants in various stages of malnutrition. Similar figures have been suggested by both Talbot[11] and Holt,[6] but both these have confined themselves to the requirements of the normal infant, and although the total requirements suggested by Talbot agree fairly closely with those suggested in this paper there are considerable differences in the amounts allocated to the various processes the sum of which makes up the total requirements.

It has long been recognized that infants should be given quantities of food suitable for their age rather than for their actual weight[10]; in other words, they should be fed according to the weight they should be for their age (the expected weight) rather than according to their actual weight. Studies of the basal metabolism[2] have confirmed this view up to a certain point, for it has been shown that the basal metabolism of infants who are within 35 per cent. of the expected weight is as high as that of infants

of normal weight. When, however, more than 35 per cent. of the expected weight has been lost the basal metabolism rate is found to decrease *pari passu* with the degree of emaciation. It seems probable, therefore, that in the graver stages of emaciation a smaller amount of nourishment should be given than the amount necessary for a well nourished child of the same age (Fig. 1). In estimating the food requirements other factors beside the basal have to be taken into consideration. These are the amount lost in the excreta, the amount added to the body in growth, and the amount used in muscular exertion.

Basal Food Requirements.

The basal requirements of normal children under 1 year of age are 55 calories per kilo of expected weight. After one-third of the expected weight has been lost the basal requirements grow less, and, as shown in Fig. 1, when an infant is only half the weight it should be the basal requirements are only about 40 calories per kilo of expected weight. This curve has been compiled from a large number of observations made by Benedict and Talbot[1] and the author,[2] on children in all stages of nutrition ranging from 20 per cent. above normal weight to 70 per cent. below normal weight.

Amount of Food Lost in the Excreta.

This is usually computed at 10 per cent. of the basal requirements, but is dependent to some extent on the quantity of food taken. Consequently in emaciated infants who are receiving a diet adjusted according to the expected weight but who have a relatively low basal metabolism on account of emaciation, this will possibly be considerably more than 10 per cent. of the basal, and 20 per cent. of the basal requirements is the figure suggested for infants who are 70 per cent. below the expected weight. This is shown in Fig. 2.

Amount of Food Required for Growth.

Only the actual amount of food stored in the body is included under this heading. If there is any expenditure of energy in manufacturing new tissue this must be included in the basal figures, for it is extremely improbable that a child ceases to grow while it is in a calorimeter having its basal metabolism examined. With normal growth it can be calculated that during the first year of life tissue of an average value of about 5 calories per kilo of weight is added daily to the body of the growing infant.[3] Emaciated infants who are gaining weight rapidly may add larger amounts, so the requirements under this heading for emaciated infants have been increased slightly, and at 70 per cent. below expected weight 10 calories are allowed for this (Fig. 2).

Amount of Food Required for Muscular Exertion.

It is impossible to give any hard and fast figure for this, but it is probable that for the healthy child about 35 calories per kilo of expected weight are sufficient. Talbot[12] examined the metabolism of two children, each for about twenty-two hours out of twenty-four, and he found that the heat output for twenty-four hours in the younger was 68 calories per kilo of expected weight, and in the older 55 calories per kilo of expected weight. If the probable amounts lost in the excreta and added to the body in growth be also taken into account the heat output of the younger is found to have been 78 calories and the older 62 calories per kilo of expected weight. These figures are surprisingly low, but it is probable that, confined as the infants were in the respiratory chamber, a considerably smaller amount of energy was expended than if they had been living their normal life. By muscular exertion, crying, etc., a normal infant raises its metabolism greatly, and an increase of 65 per cent. above the basal has been allowed for this. This entails an addition of 35 calories per kilo of expected weight, and brings the total requirements to 100 calories per kilo of expected weight (Fig. 2). It must be remembered, however, that the atrophic infant does not show the same vigour as the healthy, and in view of this a smaller allowance has been given for muscular exertion as emaciation increases, so that at 70 per cent. below expected weight only 20 calories have been allocated under this heading. The sum of these four allowances gives us the total caloric requirements for twenty-four hours for children under 1 year with varying degrees of malnutrition (Fig. 2).

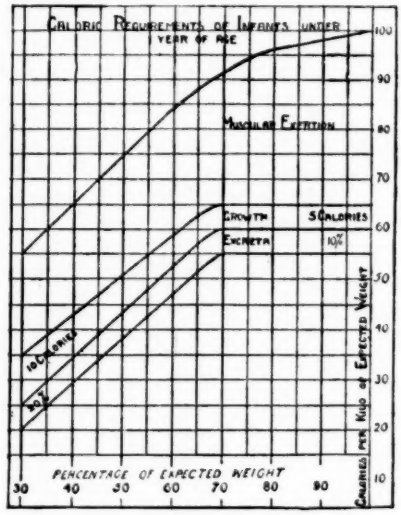

FIG. 2.—Caloric requirements of infants under 1 year of age.

Heubner[5] considered that at birth 100 calories per kilo should be given, and at 9 months of age only 70 calories per kilo. In view of the fact that it has been shown that the basal requirements are practically the same per kilo from 1 month of age till the end of the first year,[1] that the loss by excreta is practically the same, and that though there is a decrease in rapidity of growth (a small item) there is an enormous increase in muscular activity, it does not seem justifiable to give relatively less nourishment to infants of 1 year old than to infants of 1 month old. It must be borne in mind, however, that there are great individual variations, and, moreover, that the calculations presented are not exact, especially regarding muscular exertion, for there must be a variation of at least 25 per cent. in the muscular energy expended by infants. For this reason some infants may require less nourishment than others; few, however, should require more than the amounts suggested.

The nature of the food and the balancing of the diet have not been considered in this paper. Only the caloric requirements based on metabolic findings are dealt with; for until these are established there can be little use in discussing whether more or less carbohydrate or fat or protein or vitamin should be given. Let us first decide how much to give the infant, and then consider in what form it should be given.

Conclusion.

It is suggested that during the first year of life infants who are within 30 per cent. of the expected weight for the age should receive a diet of a value of 100 calories per kilo of expected weight per diem, and that below this a diet of gradually decreasing caloric value should be given.

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British Medical Association.

PROCEEDINGS OF SECTIONS AT THE ANNUAL MEETING, BRADFORD, 1924.

SECTION OF PATHOLOGY AND BACTERIOLOGY.

Professor C. H. BROWNING, M.D., President.

DISCUSSION ON IMMUNITY, WITH SPECIAL REFERENCE TO SPECIFICITY AND THE INFLUENCE OF NON-SPECIFIC FACTORS.

I.—R. A. O'BRIEN, C.B.E., M.D.

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IF I can contribute anything of interest to the discussion to-day it will be based upon work which has been carried out during the past twenty-five years in a large laboratory where, in addition to a considerable amount of research work, probably 100,000 closely controlled experiments have been done in connexion with the production and testing of serums, vaccines, tuberculins, and laboratory reagents. For the purpose of this paper my colleagues have generously placed at my disposal the whole of their work, both published and unpublished.

When carrying out the large number of tests mentioned the various laboratory workers have been constantly on the watch for the appearance of any factors that might affect the accuracy of the test or produce unexpected results. Apart from one exception, referred to later, we have never had any evidence that any "non-specific" factor—in the sense in which this word is ordinarily used—has affected our results. My apology for emphasizing these laboratory experiences is that they must influence my point of view; in this discussion I can represent mainly and almost solely the point of view of the laboratory worker.

When faced with a new method which appears to promise prevention or cure, the strong tendency of the laboratory worker is to say in effect, "By a certain injection or treatment I can produce in every animal certain accurately measurable symptoms or lesions, or, it may be, death. Will this new method prevent these lesions or cure the animal in an experiment repeated one hundred times?" This point of view has been present throughout my consideration of the whole problem.

It is expedient to try to define the subject we are considering. It seems to be impossible to define "specificity" concisely, because the word has been used in therapeutics in several senses. In the immunological field the word "specificity" indicates adjustment of the remedy to the infecting agent, even to the particular strain of organism. "Specificity" has also been used as between the infecting organism and the host. We all agree that diphtheria antitoxin is a "specific" remedy for diphtheria in the original immunological sense, and that quinine is a "specific" for malaria in the second sense in which the word is used. We are also all agreed in considering the use of *coli* vaccine for the treatment of typhoid fever, or the injection of milk or peptone into sufferers from asthma or arthritis, as instances of "non-specific" treatment.

Now the whole of the tests to which I referred above have been based upon the original rigid doctrine of specificity. We know that diphtheria antitoxin will save the life of a guinea-pig injected with an otherwise fatal dose of diphtheria toxin. We believe this protection occurs through a definite chemical or physico-chemical reaction between the toxin and the antitoxin. We know of no other substance which is neutralized by diphtheria antitoxin. We know of no other substance which will neutralize the action of diphtheria toxin and that alone. We believe also that antitoxin cannot be produced except as a result of the action on the living animal of a small dose of toxin. We believe that we can detect 0.00000025 c.cm. of a "high potency" diphtheria antitoxin by the intradermal method. If a minute quantity of diphtheria toxin is injected into a guinea-pig's skin it will produce within twenty-four hours a red flush with slight thickening of the skin, but if a minute amount of antitoxic serum be added to the toxin before injection the toxin will be neutralized and the flush will not appear. Similarly we believe that "tuberculin" injected into a tuberculous guinea-pig will produce a striking fall of temperature and death, the autopsy showing characteristic engorgement of the tuberculous tissue nearest the injection, engorgement of spleen and liver and of all tuberculous foci in the body. I know of nothing that will produce this picture but "tuberculin"—that is, some product of the tubercle bacillus.

It may be of interest to recall the problem set the laboratory early in the war, when tetanus and gas gangrene appeared in wounded men. It was easy to produce antitetanus serum which would protect an animal against the attack of tetanus otherwise inevitable after the injection of ordinary soil containing tetanus spores. It was soon discovered that *B. welchii* was the cause of much gas gangrene, and a serum was produced, but it was found in the laboratory that if one obtained material from cases of gas gangrene and injected it into animals protected with antitetanus serum and anti-*welchii* serum, an occasional sample produced gas gangrene in the protected animals. Further research showed that the organism responsible was the *Vibrion septique*. The problem of making anti-*Vibrion septique* serum was attacked and solved. But still it happened from time to time that an animal injected with material from some case of gas gangrene died of gas gangrene, even though protected with antitetanus, anti-*welchii*, and anti-*Vibrion septique* serum; from these animals another organism, *B. oedematiens*, was obtained and an antiserum prepared. In 1918 it was possible to provide in the laboratory a serum a few cubic centimetres of which would protect an animal injected with mixed cultures of *B. tetani*, *B. welchii*, *Vibrion septique*, and *B. oedematiens*. The whole of this work in several allied laboratories was based on the rigid laws of specificity.

I may recall another interesting instance of specificity. We repeatedly make a serum 1 c.cm. of which will protect a mouse against 50 million fatal doses of living pneumococcus Type I; yet we find that this serum will not protect a mouse injected with a small quantity of Type II pneumococcus, which is indistinguishable by morphological or biochemical reactions from Type I pneumococcus. There are at least two, probably three, groups of *B. botulinus* indistinguishable except by agglutination test and by the capacity to produce serum protective only against the homologous bacillus or toxin. Apparently there are two types of the virus of foot-and-mouth disease, for immunization with one will apparently protect against the first strain only and not the second. The foregoing are instances of rigid specificity in connexion with infectious or bacterial disease.

Up to the present we have been considering only phenomena based on rigid specificity. It was long before the laboratory worker found any phenomena reproducible with certainty and accurately measurable in which these rigid laws of specificity appeared to need some relaxation or redrafting. There are a series of such phenomena. One of the most familiar is the Wassermann reaction; the serum of the syphilitic patient reacts with an antigen made from the heart muscle of a healthy human being or an animal that has not been in contact with syphilis. It is of interest that in the earliest application of this test to the diagnosis of syphilis it was considered necessary to use a specific extract of the liver of a syphilitic foetus. When a patient suffers from typhus fever, agglutinins of the X 19 bacillus appear, though there is no certain causal relationship between this bacillus and the disease. In the Forssman phenomenon we find that an injection of guinea-pig kidney tissue into a rabbit causes the appearance of antibodies which will lyse sheep red blood cells. We may explain this by assuming that an essential antigen to sheep red cells happens also to be present in kidney of guinea-pig, horse, etc., but *not* in guinea-pig red cells, etc.

While no substance other than specific antibody may neutralize a toxin, or any other substance than toxin cause

production of antitoxin, the *availability* for neutralizing action, of antibody already formed, may be influenced in several ways. It was long ago shown that an animal whose blood over a long period contained a certain stable concentration of tetanus antitoxin may lose in repeated bleedings as much antitoxin as was contained in the total blood at the commencement of the experiment, and yet, without further injection of tetanus toxin, it may, after the bleeding, replace in the circulation as much antibody as was abstracted. Glenny and Sudmersen[1] showed that a guinea-pig immunized with diphtheria toxin maintained the same concentration of antitoxin in its blood for a period of from one to two years; during this time it almost doubled its weight and gave birth to eight young, the blood of which contained at birth the same concentration of antitoxin as that of the mother throughout the observation. It is fairly certain that this guinea-pig had produced during the experiment and without any further stimulus by toxin injection at least as much antitoxin as its blood contained at the beginning of the experiment. I have shown that from a horse whose serum is lytic for sheep red cells one can withdraw in repeated bleedings as much haemolysin as was present initially, and yet the animal without the injection of further sheep red cells will replace in the circulation—conceivably from stores in the tissue, but almost certainly by fresh production—the whole of the haemolysin abstracted. It is conceivable that other stimuli than the abstraction of blood might raise the titre of antibodies in the circulating blood, and attempts have been made to increase the production of antitoxin such as diphtheria antitoxin by the injection of agents other than diphtheria toxin. It was thought some years ago that the injection of pilocarpine would increase the yield of antitoxin from a stud of horses, but this method has not justified the original claims made. Recently Copenhagen workers have stated that the use of injections of manganous chloride has increased the yield of diphtheria antitoxin from horses. In our hands and in those of some other investigators this method has not increased the yield of antitoxin. The statement has also been made that injections of other non-specific agents—peptone, etc.—will raise the titre of antitoxin in the blood.

My colleague, Mr. Glenny, injected peptone into a series of rabbits some of which had been immunized a year or more previously and still had a small amount of antitoxin in the body or circulation. In none of these rabbits was the antitoxin concentration of the blood affected by the injection of peptone. Half of a further series of rabbits which had a basal immunity received a second injection of diphtheria toxoid—that is, a secondary stimulus—and half received the injection of diphtheria toxoid twenty-four hours after an injection of peptone. The injection of the peptone does not seem in any way to have affected the result.

TABLE I.—*Actively Immune Rabbits injected with Peptone.*

Rabbit.	Previous Injection.	Weight in Grams.	Peptone Injected.	Antitoxic Value.	
				Before.	24 hours after Peptone.
G.11	12 months	3875	9.8 c.c. 10% Witte	1/20	1/25
G.12	16 „	2725	0.68 c.c. 10% Witte	1/40	1/40
G.13	15 „	2950	9.8 c.c. 10% Armour	1/2000	1/2000
G.17	15 „	3425	1.15 c.c. 10% Armour	1/5	1/5

TABLE II.—*Effect of Peptone on Secondary Stimulus Response.*
(Actively immune rabbits injected with 5 c.c. B.1092 (toxoid) 24 hours after peptone.)

Rabbit.	Previous Injection.	Weight in Grams.	Peptone Injected.	Antitoxic Value.	
				Before.	7 days after Secondary Stimulus.
G.22	16 months	2700	0.9 c.c. 10% Armour	1/250	units per c.c. 4
G.23	16 „	2750	—	1/50	12
G.211	3 weeks	1900	0.45 c.c. 10% Witte	1/250	4/5
G.213	3 „	2100	—	1/100	1/5
G.212	3 „	2100	—	1/5	12
G.214	3 „	3175	0.13 c.c. 10% Witte	1/5	3

TABLE III.—*Effect of Intravenous Injections of Peptone on the Immunity Produced by the Injection of Diphtheria "Toxoid."*

			Number of Guinea-pigs showing an Immunity Index of—		
			12 days.	14 days.	Over 14 days.
Controls 5 c.c. B.1092 (toxoid)			0	1	2
			0	2	4
			0	2	4
			0	1	1
			0	6	11
Peptone Injected.	Dose in mg. per gram of body weight.				
Witte.	1/2 or less ...	Before toxoid	0	2	6
	1/4	11 and 13 days after	0	2	4
	1/8	11 and 13 days after	0	2	1
	1/8	11 and 13 days after	1	3	2
	less	11 and 13 days after	—	2	4
			1	11	17
Armour.	1/6	Before toxoid	0	2	1
	less	Before toxoid	0	3	3
	1/6	3 and 5 days after	0	1	2
			0	6	6
	1/3	3 and 5 days after	0	2	1
	1/3	3 and 7 days after	2	0	0
	1/6	6 days after	1	3	2
	1/6	8 days after	2	1	1
	1/6	11 and 13 days after	1	2	0
	1/6	11 and 13 days after	0	4	2
	less	11 and 13 days after	1	3	2
			7	15	8

Yet Dale and Kellaway with Cowell found that "non-specific" substances such as peptone or normal horse serum had an interesting effect on sensitized animals. These workers showed that if one injected serum protein or crystalline egg albumen into a series of guinea-pigs the animals became sensitized by the albumen, so that the well known phenomena of fatal anaphylaxis immediately followed the intravenous injection of small doses of protein; but if to a sensitized guinea-pig an injection of normal horse serum or peptone was given prior to the injection of the sensitizing protein the animal was able to tolerate the dose of albumen that killed his fellow.

Kellaway and Cowell examined and rejected the suggestion that the antibody to the egg albumen, present in the sensitized guinea-pig, is released by the action of the injected peptone from its home in the body cells and appears free in the circulation, ready there to meet and neutralize the incoming albumen. They concluded that the "desensitization" of the sensitive animal was due to some temporary alteration of the molecular constitution and reactivity of the muscle cells of the body. It may be that this experiment is one of a series of phenomena that have some general resemblance to each other. We may imagine that one can distract the attention of the antibody-manufacturing apparatus of the body so that it does not function as it would were its attention not otherwise engaged; one may, so to speak, "crowd out" a certain function of the apparatus. My colleague, Mr. Glenny, finds a suggestion of this phenomenon in several series of experiments on immunity in connexion with diphtheria toxin. If one injects a mixture of diphtheria toxin and antitoxin into a rabbit the animal will quickly become immune. If, however, one gives an injection of cow serum to a rabbit and after an interval of four weeks gives another injection of cow serum, followed four days later, when the formation of precipitin in response to the second injection of cow serum is in full flood, by an injection of a mixture of diphtheria toxin and horse diphtheria antitoxin, the prophylactic mixture now entirely fails to immunize the animal. Apparently the serum factory in the rabbit's body is so fully engaged that

it has no energy to spare to make diphtheria antitoxin in response to the injection of prophylactic.

We now arrive at the more definite consideration of measures commonly considered as "non-specific." We may here put a series of questions.

1. Do non-specific agents release protective antibodies into the circulation? I can find no clear proof based on animal experiment that they do. But one series of experiments bearing on this point may be of interest. Mr. Glenny has very kindly allowed me to quote from an unpublished paper a curious result he has obtained. If one injects diphtheria prophylactic—either a mixture of toxin and antitoxin similar to the prophylactic used in immunizing human patients against diphtheria, or a diphtheria toxoid—into a guinea-pig, the animal does not show any immunity for some little time; the length of time depends on the efficiency of the prophylactic. After a period of a few weeks the injected animal is "Schick tested" each week. At first the result of the test is positive, but after a varying number of weeks the weekly test gives a negative result. The number of weeks elapsing before this negative result is obtained is called the immunity index. A low index obviously denotes a good antigen, a high index a poor antigen. From Table III we see that diphtheria toxoid in an untreated animal gives on the average an index of 14 or higher, and animals treated with Witte peptone or small doses of Armour peptone give an index of 14 or higher; but the interesting result is that guinea-pigs receiving fairly large doses of Armour peptone gave in twenty-two instances out of thirty an index of 14 or less. It would take a great deal of work and a long series of control experiments to make certain that this result is "significant" in the statistical sense, but one cannot resist the conclusion that the lowering of the index—that is, the increase in the efficiency of the immunization—is a direct result of the injection of the peptone.

2. Do these measures act favourably on specific infection? Through the kindness of Dr. Auld and Dr. Gow we have had the opportunity of testing on animals the effect of injections of stocks of Witte and Armour peptone which had given favourable results in the treatment of human beings. These peptones were used in the laboratory in various experiments. I am indebted to three of my colleagues for some attempts to discover whether injections of these peptones could produce any effect in animals injected, together with adequate controls, with a known minimal lethal dose of various agents. I chose a typical toxin—diphtheria; the pneumococcus—cause of a typical bacterial infection; and a bacillus that acts probably partly by intoxication and partly as a bacterial infection—*B. dysenteriae* Shiga. These three agents were chosen because we are constantly experimenting with them and the exact doses of the various agents and the reactions of the animals thereto were well known to us; we could thus feel fairly confident that no obvious fallacy in the experiments would be overlooked.

Diphtheria Toxin.—A matured toxin was taken, the minimum lethal dose of which was known. Four guinea-pigs were given one certain fatal dose; all were dead by the fifth day. Four others were given Armour peptone (1/6 mg. per gram of body weight intravenously) twenty-four hours previously; all four were dead by the fifth day. The peptone had no influence on the time of death.

Culture of Pneumococcus.—Two series of experiments were carried out in which 98 mice were used. A dose of culture (0.0000001 c.cm.) was given to the whole of the first series of mice. The peptones (Armour and Witte) were given intravenously twenty-four hours after the pneumococcus culture. Of the 10 mice receiving the culture alone, 7 died; of 32 receiving the peptones after the culture, 18 died. In a second series a more virulent culture was used. Of 12 mice injected with culture 11 died; of 35 mice receiving culture followed by peptones 30 died. No significant difference between the death rate of those receiving culture alone and those receiving also Armour and Witte peptone is discernible. Three doses were chosen. The smallest corresponds fairly closely per gram of body weight with that ordinarily injected by clinicians into human beings; the largest, or "shock" dose, is near that which produces a definite temperature disturbance and obviously affects the behaviour of an animal, and is not far below the lethal dose; a dose halfway between these was also used. The figures of the two experiments do not show any influence, either favourable or unfavourable, exercised by the peptone.

Dysentery (Shiga) Toxin.—Two doses of toxin were chosen. In the first series 0.1 mg. of toxin, in the second 0.15 mg. was mixed with a standard quantity of antitoxin—that is, 0.025 c.cm. It was known that the lower dose of toxin in the toxin-antitoxin mixture would kill about 50 per cent. of mice; it killed in this experiment 3 out of 6 injected. The larger dose of toxin killed all of 6 mice injected. The toxin-antitoxin mixtures were given to two series of mice, groups of which received the three chosen doses of peptone six hours later. In the first series, of 18 mice injected 9 died; in the second, of 23 mice injected 21 died. The peptone appears to have had no influence on the results.

The net result of these experiments is that when we used animals receiving these particular doses at the times indicated, no protective or curative effect could be traced in any of these intoxications or infections. It is, of course, possible that with other doses and other time relationships different effects might have been obtained. It is also true that these infections and intoxications constituted a severe test and are not those in the treatment of which the clinician has obtained his most favourable results. We cannot easily produce in animals either chronic arthritis or conditions such as asthma, hay fever, etc., and it is in diseases of this kind that careful clinicians have convinced themselves that favourable results follow the use of these peptones. Statements are also made that the administration of non-specific agents may protect animals against various alkaloids. We have examined only one of these statements. Starkenstein[2] stated that milk would protect rabbits against a fatal dose of strychnine given about an hour later. My colleague, Dr. Trevan, took two series of rabbits, six in each. The rabbits were divided into pairs of approximately equal weights; to one rabbit of each pair was given 0.2 mg. of strychnine intravenously per kilo of body weight; to the other, 5 c.cm. of milk per kilo were given intravenously between half and one hour before the dose of strychnine. Of the six receiving strychnine and no milk, four died; of the six rabbits receiving milk and strychnine, three died. The administration of the milk has apparently not influenced the result of the injection of strychnine.

3. Are non-specific measures "activating agents"? If we knew more exactly how the various infections and intoxications produced their results it would help us in our consideration of the clinical records of "successful" cases of protein therapy. To take a crude illustration: if we knew that a given toxin—for example, that causing asthma—produced its symptoms by stimulation of the vagus nerve and that a certain alkaloid x could correspondingly depress the activity of that nerve, we could conceive that the alkaloid x or a peptone containing it might cure asthma. Dale and Miss Robertson have shown that the toxin of *Vibrion septique* has a digitalis-like action and produces a great fall of blood pressure, and that antitoxin, whether given previously to the animal or mixed with the toxin before injection, prevents this fall of blood pressure.

But, unfortunately, we have very little accurate knowledge on which to base hypothesis. If we knew that the occurrence of a leucocytosis would favour cure in pneumonia, we could readily see that the injection of substances of the nuclein and the tallianine class, which cause a leucocytosis as well as an increase in opsonins, might act advantageously in the treatment of pneumonia. Unfortunately we have no certain evidence that the injection of such substances will protect or cure either experimental animals or human beings suffering from pneumonia.

We know that the injection of various non-specific agents may produce great changes in the blood, and changes in coagulability, in the proteins, cellular elements, and platelets. It is easily conceivable that these changes are associated with changes in some natural mechanism of defence, differing from those ordinarily recognized by immunologists.

It is tempting to entertain the suggestion that any injury to body cells, however caused, calls into play the appropriate defensive mechanism and that whether this injury to cells be caused by the injection of casein, vaccine, or peptone, or, as in the recent deeply suggestive results of Wright and his colleagues, by the reinjection of treated blood or by exposure to light stimuli, the effect is a stimulation of natural defence in which presumably the leucocytes play an important part. Defensive processes other than those ordinarily recognized by immunologists have been outlined by Dale in his suggestive discourse at the Royal Institution in May, 1922. (See also *Physiological Reviews*, iii, 3, 1923.) In considering the conception of

chemotherapy brought forward by Ehrlich, and his own researches with derivatives of emetine, Dale draws attention to the evidence indicating that Ehrlich's original hypothesis of the direct parasitotropism of the "chemotherapeutical" group of drugs does not fully explain the phenomena observed. He points out that the action of emetine compounds in the treatment of dysentery, of salts of antimony in kala-azar and bilharziasis, and of "205" and salvarsan in spirochaetal and trypanosome infections, cannot be fully explained by the effects of these drugs on the various infecting agents in the test tube. The body cells must presumably play an intermediate and all-important part in producing the favourable therapeutic effect of these drugs; possibly a new therapeutic agent is formed by the union of the drug and the patient's cells, or a specific product formed by the body cells in response to the injection of the chemotherapeutic agent.

This mode of action we are considering is presumably essentially different from the direct physical or physiochemical union between a toxin and its antitoxin. It is possible that the injection of non-specific protein "activates" groups of defensive body cells in a manner similar to that hypothecated for the chemotherapeutic agents above mentioned.

4. Is the evidence that the non-specific agents have any favourable remedial effect clear and indisputable? Before reviewing the difficulties the clinician must face in endeavouring to prove any hypothesis it may be of advantage to pause for a moment and consider the nature of the proof demanded in an immunological laboratory. The determining of a minimum lethal dose or the test dose of a toxin or antitoxin will serve.

TABLE IV.—*Minimum Lethal Dose; Shiga Toxin AA.*
(Intravenous Injection: Mice.)

Amount of Toxin in mg.	No. Dying.	No. Living.	Percentage Deaths.	Amount of Toxin in mg.	No. Dying.	No. Living.	Percentage Deaths.
0.1	1	0	100	0.015	8	6	57
0.05	1	0	100	0.01	9	9	50
0.04	6	0	100	0.0075	5	7	42
0.03	5	1	83	0.005	4	12	25
0.025	13	3	81	0.0025	0	8	0
0.02	13	3	81				

TABLE V.—*Determination of "Test Dose" of Toxin AA.*
(Intravenous Injection: Mice. Antitoxin C.950, 0.025 c.c. constant.)

Amount of Toxin in mg.	No. Dying.		No. Living.		Percentage Deaths Seventh Day.		Percentage Deaths Fourth Day.
	R.	S.	R.	S.	R.	S.	R.
0.25	22	—	0	—	100	—	100
0.2	35	14	0	0	100	100	88
0.15	32	21	3	1	93	95	68
0.125	15	16	5	2	75	84	55
0.1	11	8	9	11	54	44	20
0.05	3	3	5	10	37	23	37

R. and S. were two observers working independently in separate laboratories.

Tables IV and V show the results obtained in endeavouring to determine the lethal dose and test dose of dysentery (Shiga) toxin. Whereas we must use 0.04 mg. of toxin for 20 gram mice in order to cause 100 per cent. of deaths, it is not until we drop the dose to 0.005 mg. that we find that 75 per cent. of the mice survive. When titrating the toxin against a fixed dose of antitoxin we find that it is necessary to take 0.2 mg. of toxin before we get 100 per cent. mortality, whereas 0.05 mg. kills from 25 to 30 per cent. of animals.

Dr. Trevan has kindly allowed me to quote, from a paper he is preparing, some results of injecting insulin into mice: whereas a dose of 0.0006 mg. per 20 gram mouse may produce hypoglycaemic convulsions or death in an occasional animal (4.5 per cent.), a dose one and a half times as large will cause convulsions or death in 45 per cent., and it is not until we give a dose four times as great that we obtain convulsions or death in almost every mouse injected. If the range be thus wide when we use inanimate toxic agents, it will be much wider when using living culture for our test; in properly controlled experiments, in order to kill 100 per cent. of animals we may, then, require ten or more times as much as will kill an occasional animal. Therefore if the statement that a certain "non-specific" medicament will protect against a lethal dose of culture were made we should demand a very large number of control animals before we should be ready to accept the statement.

We realize from these figures that even where we have complete control of the animals under treatment we must proceed statistically, and, in order to prove indubitable protection or cure, must use a considerable number of experimental and control animals. In transferring his attention from laboratory experiments to results obtained at the bedside, the laboratory worker becomes vividly alive to the fact that the difficulty of proof where human beings are concerned is incomparably greater than in laboratory experiments, and that the clinician has a more difficult task than his colleague who works with animals and test tubes. The laboratory worker must pay a profound tribute of respect to the achievements of the clinician in the face of immense difficulties. But this sympathy and admiration must not allow us to lower the standard of evidence that we ask before we are convinced. We remember that the guinea-pig is insusceptible to suggestion, and that it does not know whether it is to expect amelioration or not from the material given. Some of the evidence brought forward in support of non-specific therapy will not bear any reasonable critical examination, and is presumably born of the courageous optimism of the clinician in his deep wish to benefit the sick. It is particularly in the treatment of the infections that the evidence appears to me to fail under criticism. It may be that the administration of *coli* vaccine does materially influence the course of typhoid fever and of arthritis, but we feel that we must have more evidence before accepting the claim. Remembering the effect of non-specific proteins in anaphylaxis shown by Dale and his co-workers, one is more prepared to believe that non-specific agents may have some effect in the group of diseases consisting of asthma, hay fever, the specific urticarias, and migraine. After seeing the results of cases which Dr. Auld very kindly showed me, it is difficult not to believe that the peptone injections have been the cause of the large measure of relief following on courses of injections given to patients who had suffered from persistent asthma for many years.

In reviewing the whole question we may ask ourselves, What will the future history of protein therapy be? Are current views of the real usefulness of protein therapy so firmly based that the treatment will become a permanent addition of value to our therapeutic armoury? We remember that non-specific therapy has been advocated for about two decades, and that its use is still limited to a comparatively small number of medical practitioners; one does not find in the records of the past few years any clear evidence that the mortality of typhoid fever or the severity of the attack is always favourably influenced by the use of non-specific vaccines, or that non-specific agents have been so successful in the treatment of chronic arthritis and similar diseases that we can hold out great hope of cure or material alleviation to every sufferer from this disease.

The laboratory worker must pause in self-examination and wonder whether such thoughts arise from deep-rooted orthodox conservatism and a repugnance to the acceptance of anything new; but he will feel justified in clearing himself, at least to some extent, when he remembers how avid the whole medical world is for new things of proved value in the fight against disease, the eagerness with which it has seized pituitary extract, adrenaline, salvarsan, "205," and insulin. A doubt must perforce make itself felt in our minds when we compare the slowness of the acceptance of the claims made for non-specific therapy.

In conclusion, the laboratory worker must always

remember that, while his object is to seek truth, the physician has the harder task—he must also cure his patient. When we attempt to form an opinion as to the exact degree of permanence and truth there is in the present clinical estimate of the utility of non-specific agents, we must retain an open mind and be prepared to believe in convincing evidence when it is produced, however little it may accord with the immunological explanations hitherto current in the laboratory. After all, it is difficult to conceive of a more rigid specific requirement than that the ovum of a given species will not develop unless fertilized by the specific sperm of that species, and yet Loeb's "fatherless frog" must again warn us that nothing is incredible except that which has not yet been proved.

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CHEMICAL STRUCTURE AND SPECIFICITY.

THE subject of this discussion directs special attention to the most striking characteristic of immunity reactions—namely, their specificity. By means of immunity reactions a distinction can be made between substances which, by all known methods of chemical analysis, appear to be identical. Dean,[1] in his address to this Section last year, remarked that a discussion of specificity would involve a review of the whole field of immunity, and he pointed out that the specific nature of the immunity reaction and the nature of the presumably chemical reactions underlying it remain as great a mystery as ever. It may be of interest to review very briefly some of the attempts which have been made in recent years to produce chemical evidence which would explain, or throw some light upon, the specificity of immunity reactions.

A good deal of work has been done in recent years on the composition and structure of the proteins of serum and egg albumen.[2 3 4 5] Pure preparations of these substances have been made and examined by different chemical procedures, and they have also been studied extensively by immunological methods. Analyses of the different proteins of ox and horse serum by the method of Van Slyke[6] reveal many interesting features. The analytical figures obtained for the total protein of ox and horse serum, as well as the albumin and globulins from each type of serum, reveal no points of difference between corresponding proteins; immunity reactions, on the other hand, demonstrate that these substances are essentially different. The analyses, however, show that albumin is different from globulin; the difference is particularly marked in the basic nitrogen fraction, especially in the lysine, and this difference in composition is also accompanied by antigenic dissimilarity. It has been shown by the anaphylaxis reaction that animals injected with albumin become sensitized to albumin alone and not at all to globulin, and vice versa.[7] It was also found that sensitiveness to albumin develops much later than that to globulin, and this, together with the fact that impure preparations were probably used by earlier workers, accounts for the diversity in results which they obtained. The analyses also show that pseudo-globulin and euglobulin, at least as regards their nitrogen distribution, are identical. Miss Chick[8] has suggested that euglobulin is merely pseudo-globulin united in some way with lipoids, and there is a good deal of evidence, in addition to the analyses of these substances, that this is so. When the antigenic properties of the globulins were investigated it was found that a rigidly specific sensitiveness to euglobulin occurred comparatively frequently; pseudo-globulin, however, sensitized to either pseudo-globulin or euglobulin, although sensitiveness to the former was always definitely superior. There was a certain amount of overlap of antigenic properties; whether this is due to incomplete separation of the globulins, or to a difference in the state of dispersion, due to the occurrence of lipoid in euglobulin, or to a real difference in structure small in extent yet sufficient to account for disparity in antigenic properties, has not yet been determined.

The information yielded by an analysis of protein by Van Slyke's method is necessarily limited; by its means the relative amounts of the different amino-acids are determined, but no knowledge of the manner in which these amino-acids are combined within the molecule is obtained. It is obvious that even in the case of two proteins composed of the same amino-acids in identical proportions these units may be joined together in an almost infinite number of different ways. If the specific antigenic properties of a protein are dependent at all on chemical composition, then the arrangement of the amino-acids within the molecule is probably as important as the proportions in which they occur. Any method which would enable us to ascertain the manner, or order, of linkage of amino-acids would permit of the subject of specificity being carried a stage further, and Kossel and Weiss's[9] method of protein racemization may be regarded as a stage in the study of the stereochemistry of proteins. Dakin[10] has shown that when a protein is incubated at 37° C. for three weeks in a feebly alkaline solution a number of interesting changes occur: it is no longer capable of digestion by proteolytic enzymes and is excreted unchanged in the faeces. When injected into animals it is excreted unchanged in the urine,[11] and Ten Broek[12] showed that such proteins are incapable of eliciting an immunity response. Moreover, when such a protein is hydrolysed it is found that certain of the amino-acids have been racemized, while others retain their optical activity. Dakin has given reasons for believing that the amino-acids which escape racemization are those occurring at the ends of the long peptide chains of which proteins are composed.

Dudley and Woodman[3] racemized the caseinogens from sheep by the same method which Dakin and Dudley[11] used for cow's caseinogen. Most of the amino-acids from the two proteins displayed the same optical properties, indicating a close similarity of structure. But well defined differences were observed as well. The tyrosine and lysine from cow's caseinogen were inactive, while these amino-acids from sheep's caseinogen were respectively laevo- and dextro-rotatory. Later, Dudley and Woodman[4] investigated euglobulin and pseudo-globulin from colostrum, and, in this case, no difference in the optical properties of the amino-acids could be detected. Now caseinogens show considerable overlapping in their antigenic properties, while chemical examination shows that they are structurally different. On the other hand, the globulins, as far as examination has gone up to the present, show no structural dissimilarity, but do exhibit some degree of antigenic independence. The results in these two cases, therefore, seem to discredit the view that specificity depends upon molecular arrangement. It must be remembered, however, that the racemization method reveals only one kind of structural arrangement, affecting only the edge of the pattern of the molecule, as it were. It may well be that other structural differences of greater magnitude occur which are not revealed by this method, and there may yet be discovered other methods which will throw light on the stereochemistry of other parts of the protein molecule.

Dakin and Dale[5] racemized the albumins from hen's and duck's eggs, particularly pure recrystallized preparations being used. To ordinary chemical analysis these substances, like the caseinogens and serum globulins, are indistinguishable. The racemization method showed that the optical properties of eight amino-acids were identical, but in the case of three others—histidine, leucine, and aspartic acid—definite differences in optical activity were observed. They also showed that the two albumins behave as distinct antigens in the anaphylaxis reaction—an instance of structural difference accompanied by antigenic independence.

Chemical investigations pursued on quite different lines by Avery and Heidelberger[13 14] have given results which may explain type specificity of micro-organisms. They have isolated from autolysed cultures of pneumococci the soluble substance which precipitates specifically antipneumococcus serum of the homologous type, and they have also demonstrated the presence of this substance in the blood and urine of experimental animals and lobar pneumonia patients. The purest of their preparations, derived from Type II pneumococcus, contained 1.2 per cent. of nitrogen, a trace of phosphorus, and sulphur was absent. The substance is essentially a polysaccharide, although not absolutely pure, and yields 79 per cent. of glucose on

hydrolysis. A precipitate was formed with Type II antipneumococcus serum only; the substance is incapable of stimulating antibody production, though serologically it is sharply type-specific. They have also prepared the bacterial protein of the same type of organism, and have found that this is group-specific but not type-specific, since it precipitates equally well antipneumococcus serum of all three types.

The chemical evidence as yet available to explain specificity is not large. The few instances given show, however, that a beginning has been made, that methods of attack have been devised, and that the possibilities of future developments and increase in our knowledge of this subject, so full of fascinating interest to the chemist and the biologist, are not inconsiderable.

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THE SPECIFICITY OF ACQUIRED IMMUNITY AND NON-SPECIFIC FACTORS IN IMMUNIZATION.

THE specificity of acquired immunity has been one of the basic principles in immunization, and, while this principle has dominated the study of immunity and its applications, non-specific factors have long been recognized in the resistance of the tissues to microbic infection—for example, the reaction of inflammation, phagocytosis, the normal opsonin and alexin of blood serum. Resistance, both natural and acquired, may represent, not one reaction between host and parasite, but the summation of various factors, certain of which we still imperfectly understand.

The specific nature of acquired immunity and the high degree of specificity of antibodies are unassailable facts in any consideration of the subject under discussion. On the other hand, granted that immunity even partly depends on a non-specific mechanism, it is only reasonable to suppose that non-specific stimuli may influence resistance. At the same time, the possibility of developing a specific immunity by heterologous effects must also be considered in view of the recognized occurrence of the so-called heterogenetic antibodies.*

The clinical evidence that has been accumulated in support of "non-specific immunization" can hardly be dismissed simply because the data do not always supply the most convincing scientific proof, though undoubtedly the results of such procedure may easily lead to fallacious interpretation. It must be noted that non-specific immunization has been applied almost entirely for therapeutic purposes—that is, as an additional influence where the primary stimulus to immunization (the infecting organism or its products) is insufficient immediately to control the infection. Moreover, the effects on metabolism produced by the parenteral introduction of substances used for the purpose may profoundly alter the chemical reactive processes of the tissues to the invading organism, on which the specific immunization depends. The whole question merits practical analysis by laboratory and experimental tests. In this contribution to the discussion I shall confine myself to those aspects of the subject with which I am familiar from personal observation.

In laboratory work proof of specificity on the one hand or of non-specific immunization on the other might be based on *in vitro* immunity reactions or animal protection experiments. It might be shown that specific serum antibodies are produced or increased in amount in response to some heterologous antigen or other non-specific stimulus. The question, of course, arises in this connexion as to whether antibody production is equivalent to immunity. Antibodies may not invariably represent the actual mechanism of specific resistance, but they undoubtedly indicate the reaction of the tissues to the particular antigen which is the essence of acquired immunity. By animal experiment increased resistance to an infection, resulting from some heterologous stimulus, might possibly be demonstrated; but data indicating such effects must be sufficiently uniform, the experiments and controls sufficiently numerous, and the test dose of virus of such potency as to exclude fortuitous results that could be fallaciously interpreted as non-specific effects due to the variation in the action of the virus on different individual animals, especially when inoculated in small doses. On the other hand, if the test dose is excessive, even an increased resistance may be overcome and negative results under these conditions would be of no significance (see Harvey and Iyengar).

Antibody reactions illustrate in the most striking fashion the specific nature of immunity, and the specificity of antibodies constitutes one of the most outstanding of biological phenomena. Specificity has been extensively studied in terms of these reactions, and on this basis antibodies were originally regarded as specific for the biological species, as exemplified in the typhoid-paratyphoid group. On the other hand, antibody specificity may be more restricted, and, as is well known, may be limited to "serological types" within a recognized species, which do not differ when tested by other criteria. Extreme limitation of specificity is seen where antibodies are specific for the individual strain, as in the case of the "typical" coliform bacilli (Mackie). Thus antiserums agglutinating the strain used for immunization in high titres may have relatively little effect on strains found to be identical by other methods as well as on allied strains exhibiting different biochemical characters. The same individual specificity is true also for agglutinin-absorption tests. Among other subgroups of coliform bacilli, however, specificity is not restricted to individual strains. Specificity of agglutinating antibodies for individual strains has also been recorded in other groups—for example, the haemophilic bacteria (Maitland and Cameron).

An interesting illustration of restricted specificity has been observed among the so-called paracholera vibrios, though not so limited as in the case of the typical coliform bacilli. While a number of serological types could be recognized (represented by multiple strains even from widely different sources), a considerable proportion of individual strains in the series studied could not be classified in this way—that is, their antiserums agglutinated only the strain used for immunization, as in the *B. coli* group (Mackie).

These facts regarding restricted specificity all tend to emphasize the specific factor in immunity. Specificity, however, may only be represented quantitatively: "group action" exemplifies this. In the *B. coli* group the specificity of the complement deviating antibody for the individual strain can only be elicited by carefully adjusted quantitative tests. A marked group reaction with strains differing considerably from the homologous organism is characteristic of this antibody effect, though it is slight in the case of the agglutinating antibody (Mackie). Thus the demonstration of specificity may depend to some extent on the type of immunity reaction investigated.

While the specificity of agglutinins for the individual strain is the general rule among the typical coliform bacilli, an antiserum may exhibit agglutinins which react even more powerfully with a heterologous strain than with the strain used for immunization. Such "paragglutination" is apparently a function of the particular antigen.

The serological reactions of the dysentery group have thrown some light on the question of antigenic specificity. Thus various antigenic components may be represented by each biological type and the strains can be classified according to the preponderance of particular components. Multiplicity of antigenic components in any one species, the preponderance of certain of these over others in individual strains, and the existence of components common to widely different biological types, would explain to some extent the occurrence of serological types, individual specificity, paragglutination, etc.

* Antibodies specific for an antigen which has no obvious biological relationship to that acting as the immunizing influence.

An apparently non-specific reaction demonstrable by antibody effects is well exemplified in the case of paratyphoid infections occurring in persons vaccinated with *B. typhosus* at a time when the post-vaccine typhoid agglutinins have almost disappeared from the blood. The paratyphoid infection leads apparently to a renewed production of the typhoid agglutinins (Mackie and Wiltshire). Other infections may also produce the same effect. Thus heterologous antigens may increase the output of antibodies after the tissues have already been sensitized as it were by a specific stimulus. This, of course, does not imply that these specific antibodies can be initially produced in response to a heterologous antigen. The reappearance of latent agglutinins has also been demonstrated experimentally (Jaggi). On the other hand, a particular antigen may more readily incite the development of specific antibodies after the tissues have been sensitized by a heterologous antigen (Khanolkar).

As is well known, specific antibodies may occur in the blood serum of man and animals apart from any obvious immunization with antigen, and the so-called heterogenetic antibodies may represent an increased output of antibodies (as the result of some non-specific influence) which the tissues are normally capable of producing in minimal amount. Thus Forssman's heterophil antibody* represents an increase of an antibody naturally present in the blood. It has been shown, however, that certain antigenic substances act as powerful incitants of this antibody, and it is possible that the homologous and heterologous antigens may possess some common chemical structure on which the immunizing property depends.

The undoubted fact that specific antibodies occur naturally apart from any stimulus with the homologous antigen and that these antibodies may be increased in the blood by heterologous stimuli is of some significance in any consideration of non-specific immunity.

The increase of antibody output by non-specific effects is of great practical importance and has been studied by various workers. It is of particular interest that, apart from protein antigens, simple chemical substances injected parenterally have been stated to act in this way. Thus Walbum has claimed that metallic salts, possessing a catalytic action, when injected intravenously, exert a marked influence on antibody production. This has been disputed by McIntosh and Kingsbury and by the opener of this discussion.

With a view to investigating the influence of non-specific effects on antibody production a number of experiments have been carried out in which the influence of various substances on the natural haemolytic antibody of rabbits for sheep's corpuscles (*vide supra*) was tested. This antibody has been found to be remarkably constant in normal animals over considerable periods,† as evidenced by tests in a considerable number of controls. The active production of this antibody in the normal animal so that it is maintained at a fixed concentration in the blood is evidenced by the fact that even after successive daily bleedings, representing a total withdrawal of 124 c.cm. of blood (in a 2 kilogram rabbit), the content in the serum remained at its original value. This in itself must be taken to represent an increased output of antibody into the blood due to a non-specific stimulus. I shall only refer here at present to certain of these experiments, particularly those with the metallic salts, manganese and beryllium chloride.

The results have been variable. Manganese chloride in repeated intravenous injections has been found to produce progressive increase of the antibody up to ten times the original content. In certain of the experiments, however, the increase has been comparatively slight, in others the antibody content remained unchanged, while in some instances very marked and progressive reduction of the antibody has been noted when repeated doses were given, especially at short intervals. The same has been found to be the case with beryllium chloride, though in certain experiments marked increase of the antibody has been noted. The increase is apparently temporary, and several days after a single dose the antibody content falls to the original value. Sodium nucleinate (intravenous), an albumose preparation (intravenous), ox serum (subcutaneous), turpentine injections (subcutaneous) have been found to produce inconstant exaltation of this antibody. The inconstancy and variation in these results contrast with Walbum's claims and may explain the extreme divergence between his results and those where completely negative effects have been recorded.

Granted that such effects may occur in certain individual animals, they represent, not the production of a new immune substance, but merely the increased output from the tissues of a natural product.

Apart from non-specific effects studied in terms of antibody reactions, the experimental production of increased resistance to an infection or bacterial toxaemia by non-specific stimuli would be of obvious significance.

In view of the fact that normal serum has been so extensively employed in non-specific therapy, the following experimental observations are of some interest.

In 1917, while carrying out test inoculations in guinea-pigs with *B. diphtheriae* where control animals were injected with the particular strain *plus* a certain amount of diphtheria antitoxin, I noted that these animals were almost invariably protected against lethal doses of *B. diphtheriae* by normal horse serum as well as by the specific serum. Subsequently this effect was studied in more detail in a large number of experiments, and observations on the same subject have been recorded by others (Kolle and Schlossberger, Kraus and Sordelli, Cowie and Greenthal). While some have regarded this as a non-specific effect, it has been difficult to interpret it as such in view of the fact that normal horse serum may exert a weak neutralizing effect on diphtheria antitoxin. The possibility of the protection conferred being partly independent of the natural antitoxin has to be considered.

It was found that this protective property was not limited to horse serum, but occurred with specimens of serum from other animals. The active substance in the serum was stable at 57° C., but labile at 70° C. The effect was a comparatively weak one as compared with the protective action of a prepared diphtheria antitoxin and no protection occurred if the serum injection was delayed for even two hours after the inoculation. Occasional specimens of horse serum were apparently inactive. Another interesting feature of the experiments was the occurrence of the characteristic local ulcerative lesion in animals surviving after protection. Thus normal serum, though protective against the lethal result of the infection, had little influence on the local toxic action produced by the culture injected. Diphtheria antitoxin, however, prevented the occurrence also of the local lesion. It seemed possible that the effect might be partly independent of the specific factor, and, in a series of experiments in which the serum of one guinea-pig was injected into another simultaneously inoculated with several multiples of a lethal dose of *B. diphtheriae*, the results compared with those in an equal number of control animals which received the same dose of culture alone and died within two days showed a 50 per cent. survival, and even when the test animals did not survive there was in the majority of cases a distinct delaying effect in the course of the infection (Mackie). Here the serum of the same species was injected, not an alien serum from an animal of perhaps greater natural resistance to the infection or containing natural specific antibodies to the particular virus or its products.

While it would be unwise to draw general conclusions from such limited observations, they are still of some interest in relation to the question of non-specific factors in immunization. If they should be interpreted as non-specific effects they at least show that such influences are in no way comparable with specific immunization.

The extraordinary claim has been made that normal horse serum has proved as effective in the treatment of diphtheria as diphtheria antitoxin in a large series of comparative

* A haemolytic antibody for sheep's red corpuscles produced by immunizing rabbits with the tissues of certain animal species—for example, guinea-pig.

† Specimens of blood were withdrawn at intervals, the serum was separated after twenty-four hours and heated for one hour at 57° C., kept in the ice chest, and ultimately tested with the same specimen of corpuscles and complement.

cases (Bingel). The experimental results I have observed only tend to show the striking difference in the effect of the specific and the normal serum.

To sum up, while specific immunization must be regarded as the most effective method by which tissue resistance is increased, the development of an acquired specific immunity or the output of specific antibodies may be influenced secondarily by non-specific or heterologous effects.

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So much has already been said by the previous speakers that I feel I have but little to add to this discussion. A review of the subject from a slightly different angle, a summary of what has already been dealt with, and a correlation of different aspects of specificity in immunity may not, however, be out of place.

Using the term "specific" in the limited sense implied in immunology, specificity is made use of in two ways in the study of disease: (1) The diagnostic—to assist in the accurate diagnosis of communicable disease; to establish the relationship between a given disease and the micro-organism held to be responsible for its causation; to differentiate one micro-organism from another; and to differentiate between proteins derived from different species, especially different animal species. (2) The second application of the specific reactions of immunity in practical medicine is the direct outcome of these diagnostic applications, and makes use of them in the preparation of vaccines and serums for the prophylaxis and therapeutics of infectious disease.

In the correlation of these two applications of the science of immunology there exists a debatable ground which is of especial interest in practical medicine. Before we attempt, however, to correlate these two applications of immunity it is necessary to realize that specificity in yet another sense altogether has been foisted upon us because of the way in which the science of immunology has evolved. I refer to the assumed specificity of structure of antibodies called forth by the inoculation of true antigens into living animals. This assumption was the central feature of the side-chain hypothesis, and according to this view the structure—in a chemical sense—of the active principle of an agglutinating serum is different from that of the active principle of a lytic serum, and in turn different from those of antitoxic and opsonic serums. There is no ground for believing that such structural differences exist, and indeed it would seem that all true antibodies are in this chemical sense the same, while the demonstration of their presence in a given immune serum depends upon the physical qualities of the colloidal system employed in making such demonstration. In fact, antibodies appear to be not tangible entities but merely qualities, for Dr. Hartley, at the recent meeting of the Pathological Society, brought forward evidence that the parenteral introduction of a foreign protein into a living animal does not result in the production of any new specific substance, at least in the blood serum of that animal, although it does result in the acquisition of new qualities by the substances normally present in the serum of that animal; it is the acquired qualities which are specific.

Put simply, when a true antigen unites with its corresponding antibody, a "complex" consisting of both antigen and serum results, and the complex so formed exhibits new qualities which cannot be expressed as the sum of the qualities of its constituents. Using *in vitro* methods the formation of such complexes can be demonstrated: (1) By their flocculation in presence of electrolytes—agglutination. (2) By their susceptibility to solution by, or avidity for, complement—lysis and complement deviation. (3) By their susceptibility to phagocytosis—opsoninization. Theoretically all three methods should be available in any given experiment, but practically one would be employed for one purpose and another for another.

The salient fact is that in diagnostic work we make use of the reaction of the body to foreign proteins in order to determine when a particular protein is present in any substance under investigation, while the technique chosen is determined by the qualities of the protein to be investigated. The tissue reactions to the parenteral introduction of foreign proteins are specific, although certain exceptions appear to exist. These have already been dealt with—the examples I have in mind are the Wassermann reaction and the Weil-Felix test—so I shall say no more of these beyond calling attention to the fact that such reactions may be empirically specific in a strictly limited diagnostic sense. Their use for purposes of diagnosis and their further study is to be encouraged, but as such reactions bear no relationship to true immunity we should be warned by them not to translate too hastily our *in vitro* findings to *in vivo* experiments.

The plea advanced in my further remarks is, then, for a closer correlation between the diagnostic application of immunology, using *in vitro* methods, and its application to the prophylaxis and therapeutics of disease. To plead for this I propose to cite certain specific examples that are familiar to all, but I would first emphasize that micro-organisms produce disease in many ways—a truism that is liable to be overlooked because of that great superstructure of technique and philosophy which encumbers the science of immunology.

Consider, for example, diphtheria. We are all too prone to consider that this condition is solely an intoxication with the products of growth of *B. diphtheriae*. This, however, is too narrow an attitude from which to view diphtheria, for a mechanism of infection must necessarily play some part in its causation, and this mechanism of infection must precede that of intoxication. While in the instance under consideration the factor of intoxication is of greater importance than that of infection, do not let us forget altogether the latter, for in other instances it may well be of paramount import. It is because of this that the differentiation of *B. diphtheriae* into serological subgroups is of very minor importance, for, with the experimental methods at our disposal, we cannot demonstrate that the exotoxin elaborated by one agglutinogenic type of *B. diphtheriae* differs from that elaborated by another.

Please note the proviso that I have been careful to make—namely, "That with the methods at present at our disposal we cannot show that the toxins of the various types of *B. diphtheriae* differ from one another." We are compelled, when investigating such problems, to employ, as the critical reagents of our tests, small laboratory animals, and it is possible that these are more delicate or less delicate indicators of toxicity than are human tissues. In such tests our animals are only indicators of an end-point in a biological titration, and at the best they are but poor indelicate indicators; but we know that in some tests they exhibit a sufficient degree of delicacy for our needs, while in others they are not delicate enough. The point which I wish to make is that diphtheria toxin is from one point of view a complete antigen but from another point of view it is only a partial antigen. The whole antigen would consist of both the micro-organisms themselves and all their growth products—not merely those products that can pass through the pores of a filter and which are sufficiently stable to allow of storage in one form or another.

It is merely good fortune that in the case of diphtheria we are dealing with a malady in which the partial antigen exotoxin is useful in that from a practical standpoint it gives specific results in (*a*) the diagnostic application of immunity in the Schick test; (*b*) the prophylactic application of immunity in active immunization; (*c*) the therapeutic application of immunity in passive immunization. In other diseases such a moiety of the antigens elaborated by the causal germ may be valueless for these purposes, and each disease-producing micro-organism should be studied to determine (*a*) its mechanism of infection; (*b*) its mechanism

of disease causation; and (*c*) its mechanism of causation of death. Any one of these may be of outstanding significance and may be related to a particular moiety of antigen—using antigen in the wide sense implied in what I have just said—or to special combinations of antigens, and the problems of specificity which arise at every turn.

Let us take another example. *B. tetani* can be differentiated into serological subgroups by means of agglutination—that is, the protoplasm of one strain of *B. tetani* may be different from that of another; it would appear none the less that all strains elaborate the same exotoxin, so that one antitoxin can be prepared to combat all. Clearly, then, one whole antigen would consist of two moieties: (1) product of growth—a neurotoxin which is of major significance; and (2) bacillary protoplasm—of very doubtful significance. I was able to demonstrate, however, that a third factor exists in whole cultures of *B. tetani*. This is a labile growth product which has slight aggressive—or, if you prefer the term, leucotoxic—qualities, and this factor is specific to the serological types as determined by agglutination. Here again, then, we are fortunate in that the main factor of disease causation, neurotoxin, is common to all types of the bacillus, while a very minor factor—minor at least in the case of tetanus—leucotoxin, is specific to the types.

A third example is furnished by *B. botulinus*, in which specificity of organismal protoplasm, as demonstrated by agglutination and other *in vitro* methods, is mirrored in a specificity of neurotoxin elaborated during growth. The neurotoxin of *B. botulinus*, Type A, gives rise to the same lesions as that of toxin from Type B when injected into experimental animals; yet antitoxin to the product of Type A will not, so readily at least, neutralize the toxin of Type B as it does the toxin of its homologue.

If now we consider tubercle in the same way a rather striking fact emerges. One can show by using the agglutination and absorption of agglutinins tests that the protein constituents of both human and bovine tubercle bacilli appear to be the same; yet notwithstanding this similarity—one might almost say identity—the two types of *B. tuberculosis* exhibit disease-producing properties of a very different order. Tubercle, then, illustrates the converse of what obtains in diphtheria and tetanus. In diphtheria and tetanus agglutination gives results which from the practical viewpoint are too specific, while in tubercle it gives results which are not sufficiently specific; we must look, then, for the specific difference between human and bovine tubercle bacilli in some factor other than the bacterial protoplasm.

Many similar examples can be quoted, some—for example, the pneumococci and streptococci—illustrating cases in which *in vitro* methods bring to light differences which are of practical significance in that such differences have a definite bearing upon the production of vaccines for prophylaxis and serum for therapeutics; others—for example, the meningococci and gonococci—in which serological differences are probably, though not certainly, of the same significance; and so on down the scale until we come to conditions such as have already been exemplified by *B. diphtheriae* where *in vitro* serological tests applied to bacillary suspensions appear to be ultra-specific from a practical though not from a theoretical viewpoint. We cannot, at least in the present state of our knowledge, adopt an attitude of purely philosophic doubt such as is exemplified in a suggestion I have heard made by more than one of us—"that agglutination is really of no importance, being only an accidental phenomenon." If it is of no importance, then I submit that so is toxin production, and indeed so may be any of the manifold activities or qualities exerted or possessed by micro-organisms.

The present position can be explained in a sentence: the *in vitro* methods—notably agglutination and absorption of agglutinins—sometimes give ultra-specific results, but that is because the major factor in disease causation by some micro-organismal species is not resident in the micro-organismal protoplasm, but in certain products of growth, and the former may be more specific than the latter. This appears to be the case in diphtheria and tetanus, but there is more than a suggestion that the reverse obtains in the case of tubercle, in which the *in vitro* methods do not give sufficiently specific results. The failure to realize this has retarded progress in the science of immunology, and, hypnotized by the purely pharmacological conception of certain infections like diphtheria and tetanus, our outlook is in danger of becoming narrowed rather than broadened. It is very striking to observe how great a percentage of the recent work of immunologists is concerned in the investigation of what might well be termed side-shows of the toxin-antitoxin combinations. But I repeat that in the sense implied in this communication exotoxins are only partial antigens and antitoxins are only partial antibodies, and while admittedly diphtheria antitoxins and antitetanic serum have given excellent results in the field of practical medicine, there is no indication that the employment of similar products for immunization against other infections will meet with a like success until at least we know what particular antigens must be used; this depends upon an adequate investigation of the mechanism of infection, as well as of the mechanism of disease causation of any given micro-organism.

We should either attempt, then, to clear up the problem of specificity in relation to these mechanisms (Dr. Hartley's paper stresses this better than I can) or return to the Pastorian method of immunization—the use of whole culture, and preferably of living cultures, to stimulate the production of immunity.

V.—L. S. P. DAVIDSON, M.B., M.R.C.P.Ed.

(From the Department of Bacteriology, University of Edinburgh.)

The Effects of Physical and Chemical Agencies on Bacterial Vaccines.

For some considerable time it has been my opinion that physical and chemical agencies when applied to a vaccine would result in a great part of its antigenic value being lost. This assumption appears to be justified in view of the fact that generally the most efficient and lasting immunity is developed when a person or animal suffers and recovers from a disease of bacterial origin. It would appear, therefore, that an organism in its natural state is the best antigen. This has become an important practical question since the introduction of certain newer methods of vaccine preparation—for example, detoxication and defatting.

Some time ago, with a view to investigating this question, the following preliminary tests were carried out. Three rabbits were immunized with a typhoid vaccine made by emulsifying in saline a twenty-four hours' growth on an agar slope. Three rabbits were also inoculated with a "detoxicated" typhoid vaccine made from the same strain by Dr. Thomson, the originator of this method of vaccine preparation. After three graded inoculations the serums of these six animals were tested by the agglutination and complement fixation tests. As evidenced by antibody response to the different antigens, very marked diminution in antigenic value was noted in the case of the detoxicated vaccine. The small number of animals used in this experiment was insufficient to eliminate the fallacies which arise in such comparisons due to varying response of different individual animals to a particular immunizing stimulus. The whole question was therefore reinvestigated on a much larger scale.

About this time Professor Dreyer[1] of Oxford published his work on "defatted" vaccines, and in consequence new experiments were planned to compare the antigenic values of the following three types of vaccines: (1) the ordinary heat-killed bacterial emulsion in saline; (2) the detoxicated vaccine; and (3) the so-called defatted vaccine. The experiment was to include an example of the three main types of organisms—namely, acid-fast, Gram-negative, and Gram-positive. The tubercle bacillus, *Bacillus suipestifer*, strain XII of the Lister Institute, and the pneumococcus, were accordingly selected. In the short time at my disposal it is impossible to give the details and the results of the whole of this experimental work, and I propose to confine my remarks to the effects of physical and chemical agencies on the antigenic value of *Bacillus suipestifer*. This organism was selected in virtue of its natural virulence for the rabbit, which was the animal employed in these

experiments. Thus, in addition to the serological index of immunity, the demonstration of increased resistance, by means of infection experiments, was aimed at. It was necessary, therefore, to use an organism of fixed virulence to the test animal.

It is unnecessary here to deal with the theoretical aspects of antibody production, and I will confine myself merely to stating what was done and the results found. I must, however, digress slightly from the practical to the theoretical on one point as it has an important bearing on these experiments. The question arises, What significance is to be attached to the finding of antibodies, such as agglutinins and complement-fixing bodies, in the serum of immunized animals? Should these substances be considered criteria of immunity? In some cases antibodies undoubtedly constitute the mechanism of specific resistance—for example, antitoxins; on the other hand, it is well known that an animal with a high content of specific agglutinins in its blood serum may show no actual immunity to the homologous infection. This may be explained on the grounds that the immunity, though increased, has not yet reached a point at which it can successfully cope with the particular infection. Again, immunity to infection may occur in the apparent absence of demonstrable antibody reactions. Our present knowledge of the mechanism of immunity is insufficient to give definite answers to all these questions. There can be no doubt, however, that the production of antibodies in the serum of an animal depends on the reaction or interaction between the antigen injected and the actual living tissue cells. This is the essential phenomenon underlying all immunity processes. For this reason antibodies may be considered as being indicators of the antigenic values of vaccines.

Objectives of the Experiment.

The objectives of the experiment were fourfold: First and foremost, to ascertain whether the physical and chemical agencies used in production of detoxicated and defatted vaccines reduced their antigenic value; secondly, to determine whether subcutaneous or intravenous injection is the more effective; thirdly, to compare the effects of varying doses; and fourthly, to find out if there was any relation between the body weight, the dosage, and the antibody production.

The actual experiment consisted in weighing and numbering thirty-six rabbits, which were divided into three groups (A, B, C) of twelve each. Group A consisted of twelve rabbits injected with the ordinary killed bacillary emulsion; Group B of twelve rabbits inoculated with the detoxicated vaccine; and Group C of twelve rabbits injected with the defatted vaccine. Each group was divided into three subgroups of four rabbits. Each subgroup contained one heavy rabbit weighing 2,000 grams or more, one small rabbit of less than 1,400 grams, and two intermediate ones of approximately 1,800 grams weight. One rabbit of intermediate weight in each subgroup was injected subcutaneously, the other three intravenously. Rabbits in subgroup A 1 received what was considered to be a small dose of the vaccine. This was approximately a total of 50 million of the organisms administered in four weekly injections of 5, 10, 15, and 20 million. Those in subgroup A 2 received medium-sized doses totalling 500 million in four weekly injections of 50, 100, 150, and 200 million; while those in subgroup A 3 received doses totalling 5,000 million in four injections. The object of this was to show that there is an optimum dosage below which smaller amounts of antibody are produced, and above which, owing to the toxicity of the foreign protein injected, there will be not only a decreased antibody production, but, in addition, illness and even death of the animals. An analogy might be drawn from a very severe case of pneumonia, in which the overwhelming amount of toxin paralyses the haemoblastic tissues in the bone marrow and thus produces a leucopenia instead of the usual leucocytosis. It is necessary to emphasize this point, because the whole foundation of the theory of detoxicated vaccine depends on the following supposition as published by Dr. David Thomson[2] in his recent book on gonorrhoea. He states that when an amount of sheep's red blood corpuscles equivalent in weight to the dose of a bacterial vaccine is injected into a rabbit, no haemolytic antibody can be subsequently demonstrated. It is only when large amounts, such as 2 or 3 c.cm., of red cells are introduced that a high titre haemolytic serum is produced. From this he draws the conclusion that if large quantities of bacterial protein were inoculated into a human being a high degree of immunity would result. In order to obviate the toxic effects which result when large quantities of bacterial protein are injected, he originated the process now known as "detoxication." It can be proved experimentally, however, that when an amount of bacterial protein is introduced into an animal beyond the optimum the antibody production falls instead of rising.

Experiments with the Ordinary Heat-killed Bacillary Emulsion.

The vaccine used in the injection of the twelve rabbits in Group A was made by growing the organism on 6 in. Petri dishes containing nutrient agar. The twenty-four hours' growth was removed by a special suction apparatus from the surface of the plate, dried, and weighed. An even emulsion in 0.5 per cent. carbol saline containing 1 mg. of dried organisms per cubic centimetre was made. This was then standardized by Brown's[3] opacity method. The approximate number of bacilli per cubic centimetre was found to be 1,000 million. This vaccine will in future be referred to as the bacillary emulsion. The remainder of the bacterial mass was used for making the defatted vaccine. Four injections of the bacillary emulsion were given at seven days intervals, and before each injection blood was removed from the ear vein of the animal and tested.

Practically all the antibody tests were tried, and it was decided that the agglutination and complement fixation tests were the most satisfactory for a comparative estimation of antibody production.

1. The *agglutination test* was performed with a marked capillary pipette, each transference from tube to tube resulting in a doubling of the serum dilution. Dilutions of serum were made from 1 in 10 to 1 in 20,000.

2. The *complement fixation test* was carried out according to Harrison's method as published in the Medical Research Council's pamphlet on the Wassermann reaction. This technique is used in the Royal Infirmary, Edinburgh, and has always been found to be absolutely reliable. The quantities used were: Antigen, 0.2 c.cm. of an emulsion of the organisms in saline equivalent to about 500,000 per c.cm.; this quantity gives good fixation without being anticomplementary. Serum, 0.1 c.cm. of undiluted serum, *plus* 0.1 c.cm. of saline, making a total quantity of 0.2 c.cm. Complement, 0.2 c.cm. of guinea-pig serum in various dilutions as found by previous titration to represent 2, 3, 5, 7, 9, 10, and 12 units of complement. Red blood corpuscles, 0.2 c.cm. of a 2 per cent. suspension of washed sheep's cells sensitized with five haemolytic doses of Burroughs and Wellcome's immune body; the total quantity in each tube was therefore 0.8 c.cm. The tubes were incubated at 37° C. for one hour to allow of fixation; sensitized sheep's cells were then added and the tubes returned to the incubator. Readings were taken half an hour and one hour later. In addition, serum and antigen controls were always included. The results of these tests are shown graphically.

Results of Agglutination Tests.—On examining the curves in Chart 1 representing the rabbits in subgroup A 1, it is noticed that the agglutination titres are low, reaching on the twenty-eighth day to approximately 1 in 1,500 in the case of rabbit 4. The lowest titre was found in rabbit 1, which was inoculated subcutaneously, and which received a total of 355 million organisms, as compared with 50 million injected intravenously in the other three. It is noticed also that the graph shows a steady rise in the titres up to the twenty-eighth day. Turning to subgroup A 2, which received medium-sized doses, it is seen that on the twenty-first day—that is, after three injections, and before the fourth was given—rabbits 5 and 6 had an end titre of more than 1 in 3,000. Rabbit 8 had died previously from a staphylococcal septicaemia due to infected scratches received from fighting, and in consequence is omitted from the

experiment. Rabbit 7, which was injected subcutaneously with a total of 750 million organisms, as compared with 500 million which the other animals in this subgroup received, had only reached a titre of 1 in 400. With the exception of rabbit 6, which had remained stationary, all the agglutination titres had fallen by the twenty-eighth day to one-half, showing that three injections were sufficient, and the fourth had merely lowered the animals' immunity. In subgroup A 3 it is observed that on the twenty-first day rabbit 9 showed an agglutination titre of 1 in 12,000, but on the twenty-eighth day it had fallen to half. Rabbit 12, which was injected subcutaneously, had reached a titre of 1 in 16,000 on the twenty-first day, which also had fallen to less than half by the twenty-eighth day. Rabbit 10 died after the third injection, and rabbit 11 after the second. One of these was replaced by another rabbit, and it also died after two injections. These animals were obviously suffering from toxaemia, and survived only two or three days.

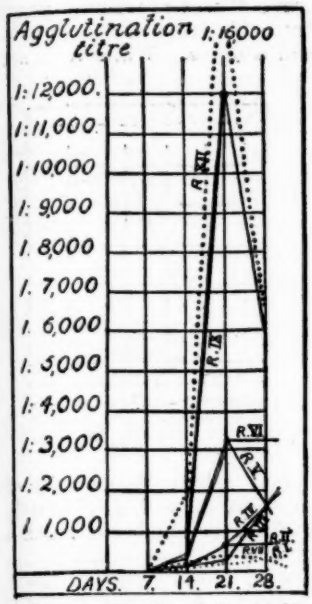

CHART 1.—Graphs of the agglutination titres of rabbits injected with a heat-killed emulsion of *B. suipestifer*.

The conclusions to be drawn from this graph are, I think, as follows:

1. Rabbits in subgroup A 1, which received the smallest dose, were given less than the optimum amount.

2. The rabbits in subgroup A 2 had received the optimum dose, whereas in subgroup A 3 three out of four rabbits injected intravenously died, showing that they had received more than the optimum dose. The fourth rabbit, which withstood the injections, however, produced the best result of all, with the exception of rabbit 12, which received large doses subcutaneously.

3. It is clear that, with the exception of subgroup A 1, in which insufficient quantities had been given and in which the titre was still rising on the twenty-eighth day, all the other rabbits would have had a higher degree of immunity with three injections instead of four, and this is shown by the marked drop in the agglutination titre of the twenty-eighth day.

4. It is evident that subcutaneous injections of small or moderate amounts do not produce as high a titre as do the injections of similar quantities intravenously. It would appear, however, that the best results are obtained when large quantities are inoculated subcutaneously, as it will subsequently be seen that rabbit 12 was the only animal to survive a dose of the living organisms. It is necessary, however, to confirm this observation, as it is of importance to know definitely whether large subcutaneous doses produce the best true immunity.

Results of Complement Fixation Test.—The results of the complement fixation test were as follows: In subgroup A 1 four units of complement were fixed on the twenty-first day, and this had fallen to three by the twenty-eighth day. Subgroup A 2 fixed eight units on the twenty-first day. The same fixation was found on the twenty-eighth day. Subgroup A 3 fixed only five units, and this had fallen to three on the twenty-eighth day. Rabbits 10 and 11 were charted until their death. Rabbit 12, which had been inoculated subcutaneously with large doses, and which had produced the highest agglutination titre, showed a complement fixation of six units on the twenty-first day and over ten on the twenty-eighth day. This confirms to a striking degree the efficacy of the injection of large subcutaneous doses.

Experiments with Detoxicated Vaccine.

The second part of the experiment, in which twelve rabbits were injected with the detoxicated vaccine, will now be considered. A culture of the organism was sent to the Pickett-Thomson Research Laboratory, and a detoxicated vaccine was prepared by Dr. David Thomson. I should like to thank Dr. Thomson for his kindness in providing me with the large quantities of detoxicated vaccines used in these investigations, and for the continued interest which he has shown throughout the course of the investigation. After the original experiment with the *B. typhosus* vaccines referred to at the beginning of this paper, I informed Dr. Thomson of the effects of his process on the antigenic value of his vaccine, and suggested that the chemicals employed were the cause of the deterioration. He himself had evidently come to this conclusion, because he had introduced a smashing and breaking machine which enables him to use much weaker chemicals. At one time detoxicated vaccines consisted of alkali, alcohol, and chloroform-soluble fractions of the bacterial protein. The chloroform-soluble fraction was first discarded, and later, as a result of investigations into the effects of alcohol on red blood corpuscles used in the production of haemagglutinins, he decided to dispense with the alcohol-soluble fraction as well.

In order to demonstrate the effect of alcohol on bacterial antigens I obtained from Dr. Thomson two vaccines, one which contained the alkali-soluble fraction only, and the other which was a mixture of the alcohol and the alkali-soluble fraction. Since detoxicated vaccines are given in doses of from a hundred to a thousand times that of ordinary vaccines, the animals in group B received a similar multiple compared to those in group A. In subgroup B 1, rabbits 13 and 14 received a total of 35,500 million organisms of the alkali + alcohol-soluble detoxicated vaccine; rabbits 15 and 16 received the same quantity of the alkali-soluble portion. This was given in four weekly injections of 500, 5,000, 10,000, and 20,000 million respectively. Subgroup B 2, consisting of rabbits 17 to 20, received 355,000 million organisms; rabbits 17 and 18 received the alkali + alcohol-soluble fraction; while rabbits 19 and 20 received the alkali-soluble fraction alone. Subgroups B 1 and B 2 were all injected intravenously, whereas the four rabbits in subgroup B 3 were inoculated subcutaneously, all four receiving a total of 215,500 million in four injections at weekly intervals, as follows: 500 million, 5,000 million, and 10,000 million, and as no antibodies could be detected in the serum of the animals a fourth injection of 200,000 million was given to each. Rabbits 21 and 22 received the alkali-soluble vaccine, and rabbits 23 and 24 the alcohol + alkali-soluble vaccine.

Results of Agglutination Tests.—In Chart 2 it is seen that the base line is the same as in the previous graph, each division representing a seven days' interval. Whereas in Chart 1 each division in the vertical line represents an agglutination titre of 1 in 1,000, in Chart 2 it represents a titre of 1 in 50 only. It is observed that of the twelve animals injected only four produced appreciable agglutinins—namely, rabbits 15 and 16 in subgroup B 1, which showed titres of 1 in 340 and 1 in 150 respectively on the twenty-first day, and rabbits 19 and 20 in subgroup B 2, which gave titres of 1 in 80 and 1 in 40 respectively. These four rabbits were injected intravenously with the vaccine made from the alkali-soluble portion alone.

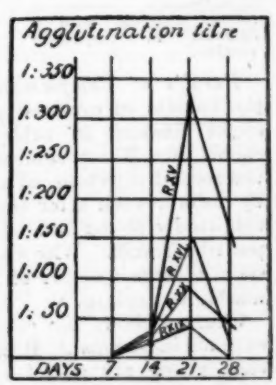

CHART 2.—Graphs of the agglutination titres of rabbits injected with a detoxicated *B. suipestifer* vaccine.

Results of Complement Fixation Tests.—With regard to the complement fixation test it is only necessary to say that the number of units fixed was always so small as to be practically negative, the average being somewhere under one unit. The detoxicated vaccine itself when used as an

antigen in the complement fixation test with the serum of rabbits in groups A, B, and C was found to have practically no fixing power.

The conclusions to be drawn from these results are, I think, quite clear:

1. That, in spite of the fact that these animals received the products of more than a thousand times the number of the organisms in the vaccine used in group A, the agglutination end titre was extremely low.
2. The effect of the alcohol is distinctly injurious to the antigen, as the rabbits injected with the alcohol + alkali-soluble vaccine produced no agglutinins at all.
3. Three injections were sufficient and by giving the fourth the end titres were reduced to half.
4. The subcutaneous injections of the detoxicated vaccines yielded completely negative results.
5. Though large quantities of vaccine in almost semi-solid condition were injected, no ill effects whatsoever were produced on the animals—that is to say, it certainly was detoxicated.

Experiments with Defatted Vaccines.

The third part of the experiment consists of the injection of nine rabbits with the so-called "defatted" vaccine. The mass of dried organisms, already referred to in part 1, was treated for four hours at 100° C. with formalin and subsequently extracted for twenty-four hours with acetone in a Soxhlet apparatus, as described by Dreyer. The defatted material was dried and weighed, and an even emulsion in 0.5 per cent. carbol saline containing 1 mg. per c.cm. was made. When examined under the microscope, after staining with carbol-fuchsin, the organisms were quite distinctly seen, and, with the exception that they seemed slightly smaller than the untreated organisms, no appreciable difference was visible. It is thus possible to compare the effects of similar doses of defatted and untreated organisms on animals.

The nine rabbits in group C were divided into three subgroups as before. Animals in subgroups C 1, C 2, and C 3 received in four weekly injections totals of 0.05 mg., 0.5 mg., and 5 mg. respectively. One of the rabbits in this last group died from the toxic effects of the injection.

Results of Agglutination Tests.—Chart 3 shows the graphs of the agglutination titres of the animals in group C. Each division on the base line represents an interval of seven days, while in the vertical line it represents a titre of 1 in 10. On the twenty-first day rabbits 25 and 27 had agglutination titres of 1 in 40 and 1 in 20 respectively, the remainder producing no appreciable agglutinins. The conclusion to be drawn from this graph is, I think, that the physical and chemical processes employed in defatting the vaccine have practically destroyed its antigenic value.

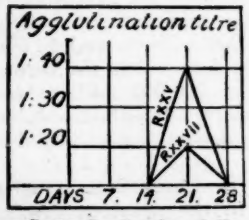

CHART 3.—Graphs of the agglutination titres of rabbits injected with a defatted *B. suipestifer* vaccine.

Results of Complement Fixation Test.—The serums of the rabbits in group C gave a negative result, except for slight fixation in rabbit 34 and very good fixation in rabbit 30. The serum of this animal fixed 3½ units after the second injection, 6½ units after the third injection, and 7½ units a week after the fourth injection. The serum was repeatedly tested and was always found to give a markedly positive result. Why the serum of this animal contained such a large amount of complement fixing antibody I am unable to explain.

Chart 4 shows the combined agglutination curves of the animals in groups A, B, and C. The agglutination titres in each group are added together and divided by the total number of rabbits. By the twenty-first day the rabbits in group A had an average agglutination titre of 1 in 4,600, dropping on the twenty-eighth day to just under 1 in 3,000, while the maximum titre of the rabbits in group B averaged 1 in 50 and in group C 1 in 5. This, I think, is a very striking picture.

Chart 5 shows the combined complement fixation curves of groups A, B, and C. The graphs are made by adding the number of units fixed and dividing by the number of animals tested. On the twenty-first day the average fixation of complement by animals in group A was 5 units, while group B fixed under 1, and group C was practically negative. The interesting point about this graph is the remarkable way in which Charts 4 and 5 agree. While it is not suggested that in any individual rabbit the graphs of agglutination and complement fixation will run exactly side by side, yet, if the average of a large number of animals is taken, the resulting curves will be very similar. An agglutination titre of 1 in 5,000 in group A is equivalent to fixation of 5 units of complement. In groups B and C the very low agglutination titres are comparable to the practically negative results of fixation of complement.

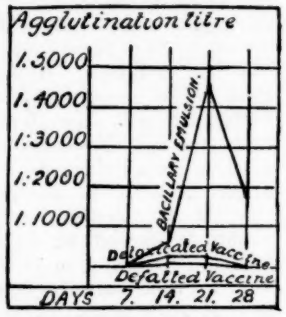

CHART 4.—Graphs of the average agglutination titre of rabbits in Groups A, B, and C.

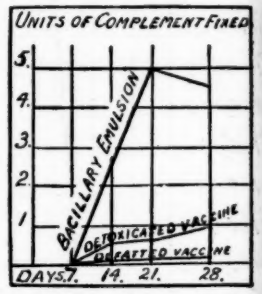

CHART 5.—Graphs of the average complement fixation of rabbits in Groups A, B, and C.

Protection Experiments.

The experiment about to be described was undertaken to ascertain whether any active immunity to a lethal dose had been produced. It was necessary first to establish the minimum lethal dose. Altogether twenty rabbits were used for this purpose. Doses of organisms from 50,000 to 1,000, the latter representing 0.0000001 mg. of dried organisms, were injected intravenously: 1,000 organisms invariably produced death from septicaemia within a week.

In dealing with experiments in which rabbits are inoculated with an organism of such high virulence as *Bacillus suipestifer*, the remarks made by Dr. Theobald Smith in a lecture on animal pathology in Edinburgh last year may be recalled. He said that some animals were so susceptible to microbic diseases that we could hope for no benefit from vaccine treatment. Again, other animals were so resistant that there was no need for vaccine treatment, while there remained an intermediary class from which good results might be expected. The action of *B. suipestifer* on rabbits comes under the first category. Pratt-Johnson,[4] when working with this organism in the Lister Institute, was quite unable to produce any actual immunity. Even so minute a number as 200 living bacilli, when introduced intravenously into previously immunized animals, proved fatal. As a result of this extremely high virulence, it has been necessary to rely mainly on the serological phenomena of agglutination, complement fixation, etc., and not on the resistance of immunized animals to lethal doses.

Six immunized rabbits from each of the groups A, B, and C were injected with living organisms: two in each group received 1,000, two 5,000, and the other two 10,000. Of these eighteen rabbits all except one died within a week and the organism was recovered from the heart blood in pure culture. The exception was rabbit 12. This animal was in subgroup A 3 and was injected subcutaneously with large doses of the bacillary emulsion. It had produced the highest agglutination titre—namely, 1 in 16,000; and also the highest complement fixation—namely, 10 units (see charts). It was a striking fact that this was the only animal that survived the test lethal dose. Moreover, it resisted an injection of 10,000 living organisms, which was 10 to 20 times the minimum lethal dose.

The conclusion to be drawn from this protection experiment is that the virulence of the test culture was of such high grade that the increased resistance produced by vaccine immunization was still insufficient to protect these

animals from lethal doses. The result obtained in the case of rabbit 12 is of some interest, but no conclusions can be drawn until the experiments at present being undertaken to repeat and confirm the facts described are completed.

Conclusions.

1. As shown by *in vitro* immunity reactions, the antigenic values of detoxicated and defatted vaccines are in no way comparable with those of the ordinary heat-killed bacillary emulsions in saline.

2. It would therefore appear that the physical and chemical processes involved in the preparation of the detoxicated and defatted vaccines are responsible for the marked loss of antigenic value.

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VI.—DAVID CAMPBELL, *M.C.*, M.A., B.Sc., M.D.,

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THE MECHANISM OF THE NON-SPECIFIC REACTION.

THE idea that an animal protects itself against organismal invasion solely in a highly specific fashion has dominated medical thought for so many years, that the suggestion that non-specific factors can play any part in the defensive mechanism of the body seems to be akin to heresy. It is true that the known facts of acquired immunity appear to warrant the conclusion that the defensive agents of the body are to be considered only in terms of specific antibodies, and that the sole basis of immunity is specificity. Nevertheless I propose, as my contribution to this discussion, to draw attention to certain clinical observations which appear to be subversive of the doctrine of strict specificity, for they suggest that the natural defensive mechanism of the body is not entirely specific in its action.

In the first place, it is not easy to see how natural immunity can be explained on a specific basis, for it is well known that many individuals are immune to certain diseases, and yet their blood contains no demonstrable specific antibodies. Nor does the clinician find in the conception of strict specificity a satisfactory explanation of the phenomenon of recovery from infection. He observes, in local infections, a series of reactions which are grouped under the general term "inflammation." It is easy to see in that process the attempts on the part of the body to neutralize the effects of the harmful agent, by increasing local metabolism, by diluting the toxin, by digesting the causative organisms, either within the phagocyte or outside the cells, or, if these efforts fail, by surrounding the irritant with a wall of fibrous tissue so that it is shut off from the rest of the body. Whether the inflammation be acute or chronic, whether it be produced by pathogenic organisms, toxins, or mechanical irritants, its basis is essentially the same. There is little or no specificity here. It is no doubt an alluring theory that, as soon as an individual is infected with the causative agent of a disease, his body cells begin to react and produce antibodies, which gradually increase in amount until finally they overwhelm the infecting organisms, and thus lead to recovery. But the clinician finds that the specific antibody titre of the serum of a patient is no guide to the possibility of recovery, and he observes that many patients overcome infections long before specific antibodies have developed in the blood in sufficient quantity to be a dominating factor in the production of this result.

In view of these observations, it seems justifiable to inquire as to the efficacy of specific biological methods in the treatment of disease. The success which followed the introduction of antitoxic serum in the treatment of diphtheria gave promise that we were at the dawn of an era of specific biological therapy. Have the hopes which were raised at that time been realized? In the light of experience, it is fair to suggest that, in the treatment of disease by antiserums, there are only two which can be said to be of any great value. Nor has the success which has followed specific vaccine therapy been any more marked. It is a curious commentary on the specificity of treatment by vaccines, that in some diseases, such as enteric fever or pneumonia, where the causative organism is definitely known, they are generally admitted not to be in the least efficacious, while in others, such as gonorrhoea, their value is, at the best, doubtful; whereas they have their greatest vogue in the treatment of catarrhal conditions of the respiratory passages, intestinal canal, and genito-urinary tract, where the task of the bacteriologist, in sifting out the true causative organism of the disease from the multitude of organisms which constitute the flora of these parts, demands a truly remarkable power of discrimination and patience.

Arising from, and because of the failure of, specific biological therapy in enteric fever, there has developed a form of treatment known as non-specific therapy or "protein-shock" therapy. The essential fact of this therapeutical measure is the intravenous or intramuscular injection of a foreign protein or some non-specific colloidal substance. This results in a general reaction on the part of the body, in the shape of a temporary elevation of temperature, increase of polymorphonuclear leucocytes in the blood stream, and, according to Petersen and his co-workers,[1] increased production of tissue enzymes. At the same time, if there is a local lesion, there is often a focal reaction, in the form of a temporary increased activity of the inflammatory process, followed by a period of quiescence. The question has been raised as to whether the evidence that this non-specific reaction has any remedial effect is clear and indisputable. It is a difficult task, even in the most accurate of sciences, to assess the value of observations made and to draw accurate conclusions from them, and the history of therapeutics shows quite clearly that this difficulty is enormously increased in dealing with the results following any remedial measure. Nevertheless, in spite of the human failing which is usually ascribed to the clinician of allowing his zeal and enthusiasm to outrun his judgement, his evidence cannot be completely ignored. During the past three and a half years I have had personal experience of treating over a hundred cases of rheumatoid arthritis by this method, and, while recognizing the impossibility of offering such proof as would be demanded in an immunological experiment, I have come to the conclusion that non-specific protein therapy is to be regarded as a valuable addition to our therapeutical armamentarium in dealing with joint diseases of this nature. It can hardly be expected to remove the adhesions from a joint, stretch a contracted muscle, or soften organized fibrous tissue, but there is no doubt that in many cases it brings the infective process to an end, and this is obviously of considerable value.

What, then, is the mechanism of this apparently illogical therapeutical measure? How can it be of benefit in disease? In an attempt to answer that question, I would draw attention to what takes place when an individual is attacked by disease. Just as in local infections there appears a series of reactions to which the term "inflammation" is applied, so, when the infecting agent or its toxin escapes into the general circulation, the body reacts in other and more general ways. There may or may not be a rigor, but the temperature in general rises above normal. There is often a decided increase in the number of white cells in the blood stream, the metabolism of the whole body is increased, and the tissue enzymes become mobilized. Are these manifestations to be regarded as purely incidental to the infection, and therefore of no special significance, or do they constitute an integral part of the body's mechanism of defence? It is admittedly difficult to answer that question. But it may be inferred from the work of Metchnikoff on the process of phagocytosis that the increase in the number of leucocytes which occurs in disease is, in some way, one of the natural methods of defence against organismal invasion. So it is with the increase in the serum enzymes. Furthermore, there is a certain amount of evidence to suggest that elevation of temperature alone may be harmful to infecting organisms. The physician is inclined to see in fever, which so frequently accompanies the invasion of the body by pathogenic organisms, something more than a mere accidental derangement of the mechanism by which the temperature of warm-blooded

animals is regulated. For he may have observed that an intercurrent acute infection, accompanied by high temperature, sometimes has the effect of clearing up a more chronic associated disease. He knows that it does more harm than good to treat pyrexia with the ordinary antipyretics. On the experimental side also, the experiments of Loewy and Richter[2] seem to show that fever is a protective mechanism, though the proof is admittedly not complete.

In this way we may begin to see something of the mechanism of the non-specific reaction. What is this form of treatment other than a method of producing artificially, but in a controlled fashion, the phenomena occurring naturally in disease, which, I suggest, are defensive in nature? At the same time, in the focal reaction, there is a temporary increased activity of any local inflammatory process, indicating an increased reaction on the part of the body locally against the disease. This was undoubtedly of benefit to the patient, for in many cases after the aggravation of signs and symptoms had passed off, there was a period when the activity of the inflammatory process was markedly diminished, and the joints became more supple and less tender. It may even be that the non-specific reaction exalts the specific mechanism of the body. It is true that the evidence on this point is conflicting, though the work of Conradi and Bieling[3] and of Hektoen[4] seems to show definitely that non-specific agents can release specific antibodies into the circulation. In any case, it is known that there may be a considerable increase in the agglutinin titre of the serum of a patient who has already specific typhoid agglutinins in his blood, if he suffers from some other acute infection, such as malaria or pneumonia. It is difficult to see how this can be explained except on a non-specific basis.

It seems likely, therefore, that the mechanism of the non-specific reaction is the general stimulation of the whole defensive forces of the body against an irritant introduced into the circulation. In a disease such as rheumatoid arthritis, the body does not, as a rule, react to any great extent against the causative organisms. There is constantly going on a warfare of low activity between the defences of the body and the invading forces of disease. When, however, a foreign protein is injected intravenously, the body reacts vigorously, and it is reasonable to suppose that, in certain cases, this reaction of the body to a non-specific irritant will result in the inhibition, or even destruction, of the microbes of the disease.

If this conception of the mechanism of the non-specific reaction is correct, it will be clear that it has certain limitations. If it is to be of benefit, a reaction must be produced, and, within limits, the improvement will vary directly with the extent of the reaction. That has actually been my experience in the use of this remedial measure. If the nature of the protein injected was such as to produce little or no reaction, no improvement followed. Yet the same patients showed marked improvement if a protein was used which caused definite reaction. Furthermore, it would be foolish to expect that a general biological reaction of this nature could cure every case of rheumatoid arthritis. Even though the whole defensive mechanism is stimulated to the full, there is always the possibility that the infective agent of the disease may resist the attack, or may be inhibited only for a short period. In any case, whether the explanation given be correct or not, the fact remains that in my experience in the treatment of this disease, the only measure which offers any great hope of quelling the infecting agent is non-specific protein therapy.

In conclusion, therefore, while it would be foolish to deny that specific factors play an important part in the mechanism of the defence of the body against disease, it seems to me that it would be equally unwise to ignore the influence of non-specific factors. The essential fact of evolution is adaptability to environment. But is it not too much to expect that an animal requires to develop a specific method of defending itself against each and every one of the infections which may attack it? Surely it is reasonable to believe that it has general methods of defence as well as specific, and that in low and chronic infections where the reaction of the body is not great, this general mechanism may be stimulated and exalted by agents which have nothing in common with the causative organisms of the disease. Such at any rate is my belief, and it is the exploitation of this general defensive mechanism that, in my view, constitutes the rationale of therapy on a non-specific basis.

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THE EFFECT OF DETOXICATION ON THE SPECIFIC ANTIGENIC PROPERTIES OF B. TYPHOSUS.*

Protein Shock Therapy.

IN connexion with the work on protein shock therapy in rheumatoid arthritis which has been carried out by Dr. David Campbell during the past few years, the protein has usually been administered in the form of a typhoid vaccine, and it was arranged that the patients' serums should be examined from time to time in order to determine how far, if at all, the relatively small doses which were being given might lead to the production of agglutinins for *B. typhosus*. As a result of the observations made in this way there are two findings which would appear to be of interest—the one indicating the damaging effect on the antigenic value of an organism produced by treatment with alkali, etc., in the process of detoxication; the other suggesting that non-specific therapy (protein shock) may evoke a specific antibody response.

In every case the test has been carried out both before and after the commencement of treatment, and, in several instances, periodic examinations have been made during its course and up to six weeks after its completion. For the estimation of the agglutinin content of the serum, the technique of Dreyer,[1] using standard agglutinable cultures and expressing the results in standard agglutinin units per cubic centimetre, has been employed exclusively.

Use of Typhoid Vaccine.

To begin with, the vaccine used was the ordinary commercial "T.V.," and the dosage consisted of one or more intravenous injections of from 100 to 200 million organisms. With only two exceptions, in a series of twelve patients treated in this way, a definite rise in the agglutinin content of the serum for *B. typhosus* was found to follow its administration, and, in the cases in which several tests were made, this rise was found to be maintained. It is to be noted that the patients who failed to show an increase in agglutinins had each received only two doses of the vaccine and that the serum test was made within seven days of the second injection.

Use of "Residual" and "Detoxicated" Typhoid Vaccines.

The work of Thomson,[2] on the lessening of the toxicity of organisms by treating suspensions with weak alkali and subsequently recovering the bacterial protein by precipitation with an acid, encouraged the hope that some of the unpleasant concomitants of protein shock might be minimized by using a vaccine which had been subjected to such a process of detoxication, and that, by injecting this relatively non-toxic bacterial protein, a simple temperature reaction might be provoked, unassociated with headache, sickness, and general malaise. With this aim in view, a "residual" typhoid vaccine was prepared, following the method detailed by Jenkins,[3] and for a time this product was utilized for the intravenous injections. The results were disappointing both serologically and clinically. No demonstrable agglutinins appeared, even after repeated doses of from 100 to 400 million organisms, and protein shock only occurred erratically. On the assumption that the temperature of sterilization might be partly responsible for these failures, several ampoules of the routine "T.V." were autoclaved at 120° C. prior to use, but such treatment was not found to impair their efficacy as agents for inducing protein shock, and, further, a second batch of "residual" vaccine, sterilized in the water-bath at 65° C.,

* This work was carried out in the Pathological Department of the University and Western Infirmary of Glasgow.

was as void of agglutinogenic power, when tested on animals, as the original autoclaved preparation. It was apparent, therefore, that the lack of agglutinin response to the injections of "residual" vaccine was attributable to some factor other than the temperature of sterilization.

More recently, two specimens of the "detoxicated" vaccine manufactured by Messrs. Genatosan, Ltd., have been given a trial. This preparation is of the mixed type, containing both typhoid and paratyphoid bacilli (T.A.B.), but with it also there has been a complete absence of antibody response, both in experimental animals and in man. No increase in agglutinins has been demonstrable in the human subject even after a total intravenous dosage of 40,000 million organisms (individual doses ranging from 100 to 20,000 million), or subcutaneous dosage of 35,000 million, while, in the case of rabbits, from 20,000 to 30,000 million organisms have been injected intravenously, in graded doses of 5,000, 5,000, 10,000, 20,000 million given at weekly intervals, without apparent harmful effect on the animals and without any development of agglutinins for any of the three organisms administered. Complement fixation tests with the serums of these patients and experimental animals have yielded consistently negative results, and no evidence has been obtained that the injection of this "detoxicated" vaccine leads to the production of any demonstrable antibodies of the type usually associated with the typhoid bacillus, certainly not with dosages which are to be considered practicable from an economic standpoint. It may be noted also, in passing, that, in the case of the two patients who received the Genatosan vaccine intravenously, doses of 20,000 million organisms failed to produce protein shock or any rise in temperature.

The results previously obtained with "residual" vaccine have since been confirmed, both in the human subject and in animals, and in all but two cases, to which reference will be made later, there has been a complete absence of agglutinin response following its injection. Rabbits have been given weekly intravenous doses of from 5,000 to 20,000 million organisms, but their serums have failed to yield agglutinins for *B. typhosus* or to fix complement when a suspension of typhoid bacilli is employed as the antigen; and these experiments have been carefully controlled both as regards the animals used and the strain of *B. typhosus* employed in the preparation of the vaccine. Used as an ordinary vaccine, this strain, when injected into rabbits of the same age and breed as those treated with the "residual" product, leads to the development of abundant agglutinins—for example, three doses of 250, 500, and 500 million yielded a serum containing over 1,500 standard agglutinin units per cubic centimetre, and giving a strongly positive complement fixation test. The "residual" vaccine was made from a portion of the same suspension of bacilli which, after sterilization, constituted the ordinary vaccine evoking this response. In addition, in order to try and obviate any fallacy arising from possible damage to the bacterial protein by exposure to the temperature of the autoclave, the "residual" vaccine for the experimental work on animals was sterilized by heating in the water-bath at 65° C. for half an hour.

It would appear, therefore, that the production of immune bodies normally associated with the administration of *B. typhosus* vaccine, or with an infection by this organism, is a highly specific reaction which depends on the presence of the unaltered bacillus, and that the process of detoxication damages the antigenic power of the organism to the extent of abolishing entirely its ability to evoke the usual demonstrable antibody response when injected into an animal. Thus, such products are not specific immunizing agents, and any antigenic value which they may possess is of quite a different order from that of the native typhoid bacillus from which they are prepared.

Specific Antibody Response following Non-specific Therapy.

Two cases in the series treated with these detoxicated vaccines are of special interest, as the serological findings are at variance with those just recorded, a rise in the agglutinin content following protein shock. This result, taken in conjunction with the evidence that has been submitted and which points to the absence of agglutinogenic power on the part of the "residual" vaccine injected, would suggest that non-specific therapy may possibly be followed by the appearance of highly specific antibodies in the blood of a patient so treated. From the history of one of the cases there are grounds for suspecting that there may have been a previous typhoid infection, but in the other such evidence is lacking.

CASE I.

J. McK., female, aged 17, was admitted to the Western Infirmary, Glasgow, on September 20th, 1921. She had suffered from infective arthritis in December, 1919, with pyrexia which lasted for several weeks. The Widal reaction at this time was reported to be positive, although, clinically, the patient's doctor did not think that the case was one of enteric fever. Improvement took place, but between March and June, 1920, there was a recurrence of the acute arthritis and the pyrexia. Similar recurrences took place in October, 1920, and in February and April, 1921.

On admission physical examination was negative, apart from the local condition of the affected joints, which showed periarticular thickening and limitation of movement. The temperature was normal, but became slightly irregular towards the end of the first week in hospital.

Details of the course of intravenous doses of "residual" vaccine administered, and of the rise in the agglutinin titre of the serum, are as follows:

Date.	Agglutinins.	Intravenous Dose.
September 29th, 1921	3.6	125 million
October 6th	3.6	200 ,,
,, 11th	355.0	250 ,,
,, 19th	714.0	250 ,,
,, 25th	205.0	250 ,,
November 3rd	—	125 ,,
,, 23rd	35.0	*Nil*

The agglutinin titres are expressed in standard agglutinin units and they were confirmed by repeat tests. On each occasion the blood for the Widal reaction was withdrawn immediately before the administration of the dose of vaccine.

Each of the first three injections was followed by protein shock, but the resulting temperature, instead of returning to normal within twenty-four hours, remained of remittent or irregularly intermittent type until the administration of the next dose of vaccine. The spleen became palpable and the affected joints were tender. After the fourth dose the temperature curve returned to normal at the end of forty-eight hours, while those following the last two doses were of the usual protein shock types. The vaccine was being given to other patients in the ward at the same time, and none of them showed any departure from normal in the resulting reactions, nor did any of them develop a rise in agglutinins. Moreover, each ampoule of vaccine contained sufficient for two doses, and the first injection administered to this patient was taken from one the remainder of which was used for a second patient whose subsequent reaction was of the usual type. Bacteriological examination of specimens of stool from the case was carried out on three occasions, but no colonies of *B. typhosus* were obtained either by direct plating on MacConkey's medium or by the brilliant-green enrichment method.

CASE II.

J. G., female, aged 27, was admitted to the Western Infirmary, Glasgow, on February 22nd, 1923. Her illness began in June, 1920, with generalized pains, involvement of various joints, and pyrexia. The patient was confined to bed for ten months, after which gradual improvement set in. Prior to admission stiffness and pain in the joints had been occasioning considerable disability. There was no history suggestive of enteric fever. Physical examination, on admission, was negative except for the local condition of the affected joints.

Details of the agglutinin content of serum and of treatment with "residual" vaccine are as follows:

Date.	Agglutinins.	Intravenous Dose.
March 1st, 1923	*Nil*	150 million
,, 5th	—	250 ,,
,, 13th	—	400 ,,
,, 23rd	58.8	400 ,,
April 3rd	58.8	*Nil*
,, 17th	—	100 ,,
,, 24th	29.4	*Nil*

The agglutinins are expressed in standard units and the titres were confirmed by repeat tests. On each occasion the sample of blood for the Widal reaction was withdrawn immediately before the administration of the dose of vaccine.

The findings in this case are very similar to those detailed in the preceding one, except that (*a*) the agglutinin titre

did not reach such a high level; (*b*) the irregular pyrexia did not set in until after the third dose of vaccine (the first to evoke a satisfactory protein shock in this patient), but it persisted for practically two months; and (*c*) splenic enlargement was not found. As with the previous case, the vaccine given to this patient was being administered to others, and, with them, the temperatures associated with the resulting reactions returned to normal in the usual short time. Cultures from the patient's urine were made on several occasions, and regular examinations of the faeces (once, and frequently twice each week) were carried out for three months, both by direct plating and by the brilliant-green enrichment method, but no typhoid bacilli were found.

The striking feature of the two cases is the appearance in their serums of agglutinins for *B. typhosus*, following on protein shock induced by a product whose specificity is at best very doubtful. Admittedly it would have been much more satisfactory if the vaccine used had been prepared from an organism other than a member of the colon-typhoid group, but it is difficult to overlook the evidence afforded by the complete failure of this same vaccine as a normal typhoid antigen in the case of eleven other patients and five experimental animals, together with the similar failure of a closely allied preparation in a further series of seven patients and three animals. There would seem, therefore, to be considerable justification for assuming that some explanation, other than a specific antigenic effect on the part of the protein injected, must be sought in order to account for the presence of these antibodies. Bacteriological confirmation of an actual pre-existing infection with *B. typhosus* is lacking in each of the cases, although in one the reported finding of a positive Widal reaction nearly two years prior to the period under review is distinctly suggestive. Did the protein shock in this case merely liberate agglutinins previously linked up with tissue cells, or did it light up a nidus of typhoid bacilli hidden somewhere in the body and thus enable them to exercise their customary antigenic effect? The continued fever, the palpable spleen, and the tender joints were certainly indicative of an active process, and, serologically, no rise in agglutinin titre was found on the seventh day of the pyrexia, although it had amounted to a high level by the twelfth. The picture is not like that of a sudden release of stored-up antibodies, and, in the few cases which have come under notice in which the patients already possessed agglutinins for *B. typhosus*, as a result of prophylactic or therapeutic inoculations, no evidence has been obtained that protein shock can exert such an action. On the other hand, it may well be that the patient in this case suffered from a recurrence of her infective arthritis, a febrile condition not necessarily induced by the protein shock, and that the rise in the agglutinin titre of her serum represented a fluctuation of the type reported by Vincent.[4]

In the other case, however, such considerations are still more speculative in view of the absence of any clinical, serological, or bacteriological evidence to support the hypothesis of a previous typhoid infection. One appears, therefore, to be confronted with a simple alternative—either that "residual" vaccine can, in exceptional cases, act as a specific antigen, or that protein shock may be followed by the appearance in the circulation of highly specific antibodies. The conclusive evidence on which to base a decision is still lacking, but, if non-specific therapy did produce a specific response to *B. typhosus* in these patients, it would appear that the possibility of a preceding infection with this organism must be taken into account and that the arthritis may have been of typhoid origin. May it not be that, in such chronic conditions as rheumatoid arthritis, the beneficial results claimed from the use of protein shock therapy are as much attributable to a stirring up of the actual focus of infection as to a stimulation of the general defensive mechanism of the body; that the ultimate factor in determining cure is the specific response to the antigenic activity of the infecting organism; and that this response is evoked not directly, but indirectly, by the protein injected?

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SOME PRODUCTS OF AUTOLYSED TUBERCLE BACILLARY CULTURES, WITH SPECIAL REFERENCE TO THEIR ANTIGENIC AND CURATIVE VALUE IN TUBERCULOUS INFECTIONS.

THIS problem suggested itself to the author in 1920, and was mainly based on the consideration of some of the well known physiological facts on enzymes in general and the specific nature of these bodies in specialized cells in particular, the specificity being purposive and depending mainly on the richness of the cells in one or another of their constituent proximate principles. Thus the hepatic cells contain glycogen, being rich in amylase, and the castor seed in lipase. Since tubercle bacilli are rich in higher fats and waxy substances which give them the acid-fast character, the question arose whether it was possible to demonstrate any specific ferments in them. Although Kendal, Day, and Walker had demonstrated a soluble lipase in the culture fluids during the growth of tubercle bacilli, they found that the bacilli themselves contained very little ferment. The problem, then, was to see if any lipase could be demonstrated in a mass of tubercle bacilli grown on suitable solid media, and, if so, what changes it would bring about on the bacillus itself.

That such a body does exist in tubercle bacilli put up for autolysis is demonstrable in a few days chemically, physically, and microscopically—as will be seen from the following table. A little later, in addition to the lipase, a

Tube.	Content.	Amount of N/100 NaOH required to Neutralize.
1	Tubercle bacilli 2 tubes + 5 drops of toluene + 0.25 c.cm. of ethyl butyrate	41.2 c.cm.
2	Tubercle bacilli 2 tubes + 5 drops of toluene + 0.25 c.cm. of ethyl butyrate (boiled)	1.4 c.cm.
3	10 c.cm. of citrated solution + 5 drops of toluene + 0.25 c.cm. of ethyl butyrate	0.8 c.cm.
4	Tubercle bacilli 2 tubes + 5 drops of toluene + 0.25 c.cm. of methyl butyrate	30.8 c.cm.
5	Tubercle bacilli 2 tubes + 5 drops of toluene + 0.25 c.cm. of methyl butyrate (boiled)	3.7 c.cm.
6	10 c.cm. of citrated solution + 5 drops of toluene + 0.25 c.cm. of methyl butyrate	3.5 c.cm.

feeble protease is also demonstrable both chemically and by the physical nature of the digest, which becomes glutinous, the bacillary portion going into an agglutinated sticky lump. With the help of petroleum ether such a digest yields three separate layers, of which the topmost two (1 and 2) are the most important, the lowest being practically the saline solution in which the autolysis was allowed to proceed.

(1) The uppermost layer is light amber coloured, and consists mainly of the fat solvent which has taken up all the lipased products of the fatty and waxy components of the tubercle bacilli, which gives them the acid-fast characters, and when evaporated after filtration yields a waxy brownish mass consisting mostly of fatty acids. This is easily neutralizable with alkalis yielding in weak dilutions a colloidal solution suitable for serological investigation. The complement fixation test made with this as an antigen has invariably yielded a negative result in a large number of serums of undoubted cases of tubercle. The antigenic properties, as tested by administering to rabbits increasing doses of this material and then testing their serums, have also yielded a negative result.

(2) The middle layer forms a soft whitish cake and consists of agglutinated tubercle bacilli, most of which are non-acid-fast, while some, though taking up the stain feebly, appear eroded as though partially altered by incomplete lipolysis. Repeated washing of this mass after drying with fat solvents yields a purified product, which is finally powdered, weighed, and suspended in saline solution in definite strength for further study as in (1). The complement fixation test with this as an antigen in a large number

of serums of undoubted cases of tubercle has yielded a definite positive result, although this is feeble as compared with the Wassermann test, and the material may be considered as feebly antigenic. The serums of guinea-pigs treated to weekly doses of 0.5 mg. have also yielded a definite positive complement fixation. These treated guinea-pigs stand the doses well, and when infected with live virulent tubercle bacilli show also a definite immunity—in some complete while in others partial, as the infection limits itself here to the proximal chain of lymphatic glands; in one instance even after eleven weeks when this mass was incised the animal recovered and put on weight, showed no trace of tuberculous lesion, while the controls died of generalized tuberculosis in three weeks to three months.

The curative value of this product was then tested in guinea-pigs, allowing a definite period for the infection to be well established. Since then this antigen has been extended to man both in glandular and pulmonary infection. The glandular cases show definite improvement, as the glands shrink to mere hard nodules, felt more than seen, provided caseation and softening have not already started. The general improvement in these cases is seen by the increase of weight and a feeling of well-being, with an accompanying improvement in digestion.

The local effect of subcutaneous injection of this material depends on the dose, as anything over 0.025 mg. produces in some a distinct local swelling and pain which takes over two weeks to subside, and even then leaves a hard nodule and discoloration of the area which may even break down with a serous discharge. A dose, however, of 0.01 to even 0.025 mg. is well tolerated. It produces occasionally a mere oedematous swelling which subsides in two or three days. The general reaction consists of a rise of temperature 1° to 2° above the usual during the first twenty-four hours. This drops to normal in two or three days. A course of at least twelve injections given weekly seems to be requisite for a definite improvement.

Memoranda:

MEDICAL, SURGICAL, OBSTETRICAL.

FATAL SPLENOMEGALIC ANAEMIA IN A YOUNG CHILD.

THE following notes on an unclassified case of fatal splenomegalic anaemia in a girl aged 3 seem worthy of record in view of the rarity of the condition.

The girl was said to have been perfectly well until the previous day, when it was noticed that the skin was yellow and she was thought to have jaundice. Her appearance was that of an advanced case of pernicious anaemia. The skin was lemon-yellow, the lips and nails nearly colourless, and the sclerotics pearly white. There was no purpura and the child looked well nourished. Marked dyspnoea was caused by movement, and the alae nasi were working. No palpable glands could be found, and the tongue and throat were quite clean. Some dark red urine had been passed, and a small amount of bile-stained fluid vomited. The bowels had acted normally the day before, and the stools were of normal appearance. The temperature was 101°, and the pulse 120, but its volume was quite good. No physical signs of disease were detected. Dr. G. F. Still saw the case for me in the afternoon. Late in the evening there was no change in the patient's condition, but she died in coma during the night. No blood examination was possible under the conditions. The urine was highly acid, and contained a few red and white blood cells and a few casts. There was more albumin present than could be accounted for by the blood, and the colour appeared to be due to a haemoglobinuria.

A limited *post-mortem* examination was done and revealed no abnormality other than moderate wasting, severe anaemia, an enlarged spleen, and blood-stained urine in the bladder. The spleen appeared to be about four times the normal size. Its capsule was smooth, and its consistency firm, but not tough; its cut surface displayed an abundant purplish-red pulp which somewhat obscured the Malpighian bodies and trabeculae. The kidneys were not altered in size; the pattern of their cortex was somewhat blurred; their cut surface had a faint brownish tint, but a portion treated with potassium ferro-cyanide and hydrochloric acid did not give the reaction for free iron. The liver was also negative to Perles's test: it was moderately enlarged and its cut surface was pinkish-grey and clouded. The bladder contained several ounces of dusky blood-stained urine, and its mucosa, like that of the renal pelves, was smooth and white. The myocardium was pinkish-grey with a clay-like appearance, and on the endocardial aspect the stippling characteristic of fatty degeneration was very faintly seen. There was no enlargement of any lymphatic glands. The lungs were aerated throughout, but somewhat oedematous in their posterior parts. The stomach and intestines were distended by gas. The bone marrow, brain, and cervical organs were not examined.

Microscopic sections were made of spleen, kidney, and liver. In the spleen there were collections of large mononuclear endothelioid cells, chiefly in the periphery of the Malpighian bodies; most of them were in advanced *post-mortem* disintegration. Many showed a swollen cytoplasm containing brown pigment, which was very abundant throughout the section, and was probably a *post-mortem* phenomenon; it did not contain iron (Turnbull's blue stain). In the remainder of the pulp cells were sparse, and there was no increase in fibrous tissue (van Gieson's stain). The blood everywhere was completely laked. In the liver there was a considerable degree of fatty degeneration, affecting chiefly the central and intermediate zones of the lobules; here and there cell necrosis was seen. There was no iron-containing pigment. The blood in many capillaries was moderately well preserved and contained no abnormal constituents. In the kidney there was advanced *post-mortem* disintegration of the cells of the convoluted tubules, but fat droplets were only present in the cells lining some of the collecting and discharging tubules. Turnbull's blue stain revealed no iron-containing pigment.

London, E.12. A. CAMPBELL STARK, M.B., B.S.

THE ELECTRICAL REACTIONS IN FACIAL PALSY AFTER SUPPURATION IN THE MIDDLE EAR.

IN the ordinary method of testing the function of the facial nerve by means of electric currents, the button electrode is placed over the trunk of the nerve and contraction from the facial muscles obtained by closing the current. This method, however, yields no indication as to the cause of the facial paralysis, but yields similar results whether the condition has originated as the result of suppurative disease within the middle ear, or as the result of the condition known clinically as "Bell's palsy." In certain cases of facial paralysis, it is important to be able to determine whether the condition has arisen as the result of a necrosis of the bony wall of the canal of the facial nerve, or whether the paralysis is due to other conditions. These points can be readily determined in such cases as can be examined within ten days of the onset of the paralysis. The test depends upon the fact that, if the necrosis of the bony wall of the canal of the facial nerve is present in sufficient amount to allow the toxic products to reach the facial nerve, the electric current will reach the facial nerve very much more easily than if the bony wall of the facial canal is complete.

In practice the test is applied as follows:

The external auditory meatus of the affected side is filled with warm saline, and an intrameatal electrode, composed of a central metal core covered in insulating material, such as rubber, of such a size that it exactly fills the meatus, is placed in the external ear and connected to a suitable source of electricity. The indifferent electrode having been placed upon the opposite arm, the current is alternately closed and broken, and at the same time gradually increased until a cathodal closing contraction of the facial muscles has been obtained, and a note is made of the number of milliampères which are required to cause this contraction. The electrode is now removed from the affected ear, and placed in the normal ear, which has previously been filled with saline, and the test is repeated.

It is found that the reaction is obtained in the normal ear with about 5 milliampères of current, but in "Bell's palsy" of recent origin from 6 to 7 milliampères are required, and that in cases of suppurative otitis media with facial paralysis of recent origin due to perforation in the facial canal the cathodal closing contraction is obtained with 1½ to 2½ milliampères of current, while in cases of suppurative otitis media without facial paralysis 5 milliampères are required.

From such cases as I have been able to examine it appears that facial paralysis does not occur as the result of suppurative otitis media unless the bony necrosis has allowed the pus to reach the facial nerve. Unfortunately it is not possible to make any very definite statement on this point, as in the last two and a half years I have only been able to examine five cases of facial paralysis due to suppurative otitis media and about thirty of "Bell's palsy," but in these cases the difference in the amount of current required for stimulation showed the above marked differences.

A. LOWNDES YATES, M.D.Lond., F.R.C.S.Ed.

Birmingham.

Reports of Societies.

FOCAL SEPSIS AND SKIN DISEASE.

ON December 4th a joint discussion was held by the Sections of Dermatology, Laryngology, and Odontology of the Royal Society of Medicine on focal sepsis in relation to diseases of the skin. The President of the Section of Dermatology, Dr. E. G. GRAHAM LITTLE, M.P., was in the chair.

Dr. H. W. BARBER, who opened for the Section of Dermatology, said that all infections, except the rare general blood infections, were primarily "focal," and led to one of two results: either they ran a definite course and killed, or produced a more or less lasting immunity; or they tended to persist or to recur, since the immunity they produced was very transient. In the latter class, with which he was chiefly concerned, were tuberculosis and infections with the streptococci, staphylococci, pneumococci, *B. coli communis*, and the various "catarrhal organisms." Of these, the streptococci and staphylococci were the most important; the former were essentially inhabitants of the mucous membranes and the latter of the skin. Different strains of either organism differed greatly in their pathogenicity, and more or less virulent streptococci played the most important part in what was known as focal sepsis. If a substance could be discovered that was as effective in dealing with streptococcal and tuberculous infections as was salvarsan in the treatment of syphilis a great deal of acute and chronic skin disease would disappear. He emphasized (1) the difference in pathogenicity of different strains of streptococci, (2) the difference in the clinical manifestations in different individuals of infection by the same organism, and (3) the fact that the same organism might pick out different tissues for attack in different individuals. Dr. Barber went on to draw an analogy between the trichophytides, the tuberculides, and (coining a word) the streptococcides, but with this important distinction—that whereas the first tended to get well spontaneously, the latter two were liable to persist owing to the development of multiple foci. In his experience it was relatively uncommon for the removal of a septic focus either in the teeth, tonsils, nasal sinuses, or elsewhere to cause a dramatic cure of a skin lesion. He believed this to be due to the fact that in most cases there was not one focus but several. Thus, a patient with septic teeth was liable to have infected tonsils, and, as Sir W. Willcox had pointed out, to have a secondary intestinal infection as well. Further, the lymphatic glands connected with all these areas might also act as secondary foci. Secondary streptococcal intestinal infection might persist long after the primary focus had been eliminated. It was essential, not only to isolate a streptococcus from the focus, but also to determine by serological tests that this was the specific infecting organism of the patient. Dr. Barber passed to the consideration of some of the eruptions which, in his opinion, might be due to focal streptococcal infection. These included (1) urticaria, of which cases had been recorded which were cured rapidly by suitable operative treatment of antral and sinus affections or by the removal of infected tonsils, (2) purpura, and (3) erythema multiforme recurrens. The last especially was due in every case he had investigated to chronic streptococcal infection. As regards lupus erythematosus, while he agreed that the chronic discoid hyperkeratotic form might be a tuberculide, he was of opinion that the majority of cases were due to the sensitization of the skin of certain areas to a particular *Streptococcus longus*. But as the disease was usually very chronic it was only very rarely that the removal of a primary focus alone would suffice to cure it. With regard to alopecia areata, vitiligo, and scleroderma, his view was that these diseases were due to the involvement of different sets of sympathetic nerve fibres. This was not always due to the same cause, but the commonest cause was acute or chronic infection. Since no specific drug existed which would destroy streptococci *in situ*, it was necessary to find and deal with all primary foci of infection. In the teeth he considered the apical dental abscesses to be of more importance than pyorrhoea alveolaris. Antral disease or sinus infection was frequently missed, as was also a focus in tonsils scarred and damaged by repeated attacks of quinsy and incomplete removal. *X*-ray examination of the alimentary tract might reveal a chronically infected appendix, or, what was no less important, intestinal stasis. The object of these investigations was to discover the actual infecting organism which could be determined by the bacteriologist by serological tests with the patient's blood. Mere removal of the septic focus was not sufficient treatment, but every effort must be made to raise the patient's resistance to streptococcal infection by all possible means.

Mr. J. F. O'MALLEY, who represented the Laryngological Section, said that while the laryngologist saw numerous cases of skin disease due to local sepsis in the ear, nose, or throat, it was only recently that cases such as lupus erythematosus, lichen planus, alopecia areata, and urticaria were referred to laryngologists for investigation of the nose and throat. The tonsil was at the present day the most popular organ in the whole domain of medicine, and in view of the fact that tonsillectomy had been very frequently performed during the past decade, some effect on the incidence of any skin diseases due to tonsillar infection should soon be visible. It was noteworthy that up to the present no effect on the incidence of acute rheumatism had been produced. He had circularized his colleagues with regard to their experience of cases of skin disease associated with focal sepsis in the nose or throat. Collectively they reported cases of alopecia areata, angio-neurotic oedema, urticaria, giant urticaria, lupus erythematosus, prurigo, erythema multiforme, and erythema nodosum, which had been either cured or very greatly ameliorated by appropriate treatment in the nose or throat. On the other hand, one or two members of the Section had been very disappointed with the results of tonsillectomy advised by their dermatological colleagues, particularly in urticaria and lupus erythematosus. Mr. O'Malley's own experience was limited to a few cases from which he was not able to claim any very striking results.

Sir KENNETH GOADBY carried on the discussion from the point of view of the Odontological Section. He said that the mouth was now regarded as the primary focus of many diseases of civilization, but there were very few skin diseases due to direct infection from the mouth—that was to say, it was very rarely that the same organism was obtained from a skin lesion as was present in the mouth. It was possible that that was due to the unsatisfactory state of their knowledge of oral bacteriology. Focal infections in the mouth might result from gingivitis, periodontal disease, and apical infections. Crowns had been unreservedly condemned by the opener, but he (Sir K. Goadby) considered that a crown properly fitted in a suitable case was preferable to a denture. The so-called apical abscess was often sterile. While it was illogical to condemn one area as the source of disease in another unless the same organisms were isolated from both, he was convinced that circulating toxins originating from the mouth did cause skin diseases, notably urticaria, erythema, and purpura. He suggested that the difference between chronic and acute disease lay in the nature of the toxins, those of the former being non-protein and so not producing an immunity.

Sir WILLIAM WILLCOX's contribution was read by one of the secretaries. He pointed out that the skin was specially sensitive to circulating toxins, as was proved by the rashes of various fevers. The commonest organism responsible for skin diseases was a streptococcus, and the common sites of the primary focus were the teeth and tonsils, from which there was a secondary implantation upon the intestine. He emphasized the importance of having a dental skiagram, which would often reveal apical sepsis when the teeth were apparently sound.

Dr. W. HUNTER said that the time was past when there was any doubt as to the role of focal sepsis in the production of skin diseases. The skin was like a tuned instrument in its seasitiveness to toxins. He was not sure that the recent fashion of grouping streptococci would help very much.

Dr. LOGAN TURNER wondered why genito-urinary and general surgeons and physicians were not also invited to take part in the discussion, since many primary foci came within their departments.

Dr. LESLIE ROBERTS pointed out that focal infection must not be confused with true infection, and added that the

certain diagnosis of focal infections by the naked eye, particularly of the tonsil, was often impossible. In this connexion he stated that the cheesy material which could so often be expressed from the crypts of the tonsils was not a definite sign of sepsis.

Mr. J. G. TURNER expressed himself a firm believer in the theory of focal infection from the mouth, and considered pyorrhoea to be a much more important factor than the so-called apical sepsis. Pyorrhoea was largely due to eating soft and sticky foods.

Dr. H. G. ADAMSON contended that there were many skin diseases for which no cause was known. So many cases got well without an infective focus being found, and on the other hand the removal of a focus of infection did not always cure the skin lesion, so that in his view the importance of focal infection had been somewhat exaggerated.

Dr. W. HILL, speaking as a laryngologist, said that he had often removed tonsils at the express desire of his dermatological friends, but without benefit in the large proportion of cases, so that he was doubtful whether it was advisable to remove the tonsils unless they showed very definite signs of disease.

Dr. ARTHUR BULLEID, Mr. T. B. LAYTON, Dr. K. SIBLEY, and Mr. LIVINGSTONE also spoke, and Dr. H. W. BARBER replied.

DIAGNOSIS OF CANCER OF THE THYROID.

THE Section of Surgery of the Royal Society of Medicine held a meeting on December 3rd. In the absence of the President, Mr. T. P. LEGG took the chair.

A discussion on the diagnosis of malignant disease of the thyroid was opened by Mr. JAMES BERRY. After giving brief details of a number of illustrations of cases and specimens exhibited, Mr. Berry proceeded to say that it was fortunate that malignant goitres were rare, since their recognition whilst still operable was often a matter of extreme difficulty. In the operable stage the only symptom often was a growing lump. Pain, dysphagia, dysphonia, and fixity of the tumour only occurred when the growth had extended to surrounding parts, and the difficulty of diagnosis was enhanced by the fact that these symptoms and signs might rarely be present in non-malignant goitres. If the great vessels of the neck were displaced by the tumour the growth was probably non-malignant, but there were exceptions even to this rule. Further, thyroid tissue might be found in any part from the thorax to the base of the tongue, and such outlying portions of the gland might be affected by malignant disease, giving rise to an unusual clinical picture. Two common forms of degeneration of simple goitres, the fibrosis and calcification of adenomata, might, by producing a localized hardness, suggest malignancy, especially as the latter might pass unnoticed by the patient for years. While the presence of a small hard lump of recent origin in the thyroid of a middle-aged patient at once gave rise to a suspicion of malignancy, a diagnosis could not be made with certainty. Mr. Berry then gave an instance of a case in which a cyst, tensely filled by recent haemorrhage, in a woman of 68, had given rise to a diagnosis of malignancy—a diagnosis supported by paralysis of one vocal cord. This case had been regarded as inoperable, and was sent to the Radium Institute for treatment. Mr. Berry's diagnosis of haemorrhage, on which he operated, was based on the smooth globular surface of the tumour and its comparative mobility. It should be remembered that haemorrhage into thyroid tumours was very common and was no indication of malignancy. Dysphagia was very uncommon in innocent goitres, even when very large and pressing on the trachea, but was an early symptom in many malignant cases. This sign also might lead to error, as in a case related by the speaker, in which the dysphagia and vocal cord paralysis persisted after the removal of a benign thyroid tumour, and proved eventually to be due to carcinoma of the cervical oesophagus. The hard, painless, bilateral goitre of middle age was often erroneously regarded as malignant and extirpated entirely. The subjects of this variety of goitre generally presented the peculiar waxy pallor of early myxoedema, which was easily recognized and was not seen in malignant goitres. In any case the practice of total removal of the thyroid was to be condemned. A number of illustrations and specimens were then shown on the epidiascope.

Mr. W. TROTTER referred to the similarity between the symptoms of malignant disease of the thyroid and of the cervical oesophagus. The symptoms of the latter in the order in which they occurred were (1) paralysis of the vocal cord, (2) enlargement of the thyroid, and (3) oesophageal obstruction. Mr. Berry had already given an instance of this similarity, and Mr. Trotter gave another. As regarded the diagnosis of operability, it should not be too readily concluded that a case was inoperable without making the attempt, for the apparent difficulties could frequently be overcome by patience and perseverance at the operation. He gave instances in which carcinomata of the thyroid already adherent to the oesophagus and the trachea had been successfully dealt with. The patients lived for five and ten years afterwards respectively. Kocher had said that where a malignant tumour of the thyroid had penetrated the capsule and secondary malignant glands were present, the outlook was very unfavourable. Yet he had operated on a girl with these conditions seventeen years ago and the patient was still alive.

Mr. A. J. WALTON remarked that the whole subject of carcinoma of the thyroid presented many curious anomalies. It had been stated, on the one hand, that the disease was so malignant that once the diagnosis could be definitely established the prognosis was hopeless. But recent statistics tended to show that the outlook was quite good. In his series of cases malignant goitres had constituted about 1.6 per cent. of all goitres. The type of malignant disease did not seem to have any definite relation to the prognosis, and, further, the pathological classification was difficult and unsatisfactory. Clinically malignant goitres might be classified as (1) secondary to adenoma or colloid enlargement, (2) apparently carcinomatous from the onset, (3) a type showing a slowly growing primary lesion, frequently overlooked, with large secondary deposits. In the first type the earliest symptom of malignant change was frequently hyperthyroidism, for which reason he regarded all adenomata with suspicion. The following signs suggested malignancy in a goitre: increase in size with irregularity of the outline and a firmer consistency, pain, and fixation to the trachea. Recurrent laryngeal paralysis was by no means an infallible sign of malignancy, nor was glandular enlargement.

Mr. T. P. DUNHILL said that the cases under consideration fell into three classes: (1) those obviously malignant; (2) those where the diagnosis was not so obvious, but based upon such signs as indefinite edges, dyspnoea, pain, etc.; and (3) a type in which the diagnosis could only be made by the pathologist after operation. He made a strong plea for the removal of all adenomata of the thyroid, since eventually they caused pressure or toxic symptoms or became malignant.

Mr. G. W. DE P. NICHOLSON spoke on the pathology of malignant goitres, and propounded the theory that the secondary deposits of apparently normal thyroid tissue which sometimes occurred in bone were not malignant, but were attempts on the part of the body to produce accessory thyroid tissue. He thought it easily possible for the active cells of a ductless gland like the thyroid to be carried by the blood stream and to give rise to fresh growths of glandular tissue in other parts, without being in any way malignant.

Mr. V. Z. COPE referred to the difficulty both in making a clinical diagnosis and in making a pathological diagnosis after removal of a doubtful tumour. He had removed a tumour from the right lobe of the thyroid of a boy aged 10 years. This was reported to be precancerous. A recurrence took place, which was reported to be cancerous. A further recurrence took place, and the boy eventually died. The sections were shown to another pathologist, who considered the tumour to be a foetal adenoma.

Mr. J. E. ADAMS and others also spoke, and Mr. BERRY briefly replied. A large collection of pathological specimens had been arranged by Mr. Berry and Dr. Scott Williamson.

THE EFFECT OF CHEMICALS ON LEUCOCYTES.

AT a meeting of the Section of Pathology of the Royal Society of Medicine on November 18th Dr. ALEXANDER FLEMING described the influence of certain chemicals on the bactericidal action of leucocytes.

Dr. Fleming first showed that the bactericidal power of blood as regards staphylococci was a function of the leucocytes, that serum had no lethal effect on these cocci, nor had blood from which the leucocytes had been removed. Leucocytes, isolated from the blood, had this power to a marked degree. The effect of the chemicals was tested by mixing known volumes of serial dilutions with volumes of defibrinated blood which had been infected with a small number of staphylococci and incubating the mixtures in "slide cells," after which the colonies which developed were counted. It was found that nearly all the chemicals tested had, in certain concentrations, a deleterious effect on the leucocytes as evidenced by the development of an increased number of colonies. In a series of salts of sodium so tested it was found that the fluoride and the silico-fluoride were especially toxic. Various antiseptics in common use were found in certain concentrations to induce an enormously greater growth of bacteria in blood. In the case of the poisonous alkaloids it required a relatively high concentration (in most cases about 1 in 1,000) to affect the leucocytes; it was shown that the two which were most toxic to leucocytes were those which were used for the destruction of protozoa in the body—namely, quinine and emetine. The bearing of the bactericidal power of leucocytes on the procedure of blood culture was discussed. While as the result of his experiments Dr. Fleming did not think that any substance would be found more suitable for addition to blood for cultural purposes than trypsin (as recommended by Douglas and Colebrook), yet this had technical drawbacks in that its sterility had always to be controlled. When sterile trypsin was not available he recommended the addition of some easily obtained chemical in a concentration just sufficient to destroy the bactericidal power of the leucocytes and yet permit every microbe to develop. Suitable mixtures for blood culture suggested were one volume of blood with one volume of 2 per cent. salt or 1 in 400 carbolic acid, or with 5 volumes of water.

Dr. Fleming also showed that with some chemicals, such as picric acid, the effect on the leucocytes as regarded their bactericidal power was more marked than was the effect on other leucocytic functions, such as the power of emigration and the phagocytic power, and was more nearly correlated to their lysozymic power.

INSULIN AND DIABETES.

AT a meeting of the Medical Society of London on December 8th, with the President, Dr. E. M. CALLENDER, in the chair, a discussion was held on the present position of the treatment of diabetes by insulin.

Dr. GEORGE GRAHAM, who opened the debate, stated that discussion of the principles of the use of insulin could now take place, whereas a year ago one was more concerned with various details. He proposed to present his remarks under five headings. First, there was the question of the aims of insulin treatment. Before the introduction of insulin one aimed, in a case of diabetes, at getting the urine sugar-free and the blood sugar between normal limits, and hence, in severe cases, low diets were essential. With the introduction of insulin it was believed that the amount of ingested carbohydrate could be increased and bigger diets employed. Dr. Graham, however, still believed in the importance of keeping the blood sugar within normal limits in order that the beta cells of the islets of the pancreas should not be overworked. This was sometimes difficult to achieve, especially when patients left hospital. The question of the presence of acetone bodies in the urine was also important, but it must be remembered that Rothera's test was a very delicate one, and small amounts of aceto-acetic acid and acetone were not of great importance. With regard to the weight of a patient one still aimed at slight undernourishment, but the difference between the normal weight and the weight maintained in diabetes was much less now than formerly. In children it was particularly important to keep them slightly under weight and not allow them to get fat. Secondly, there was the type of patient suitable for insulin treatment. It used to be said that if a patient could take 50 grams of carbohydrate and an adequate calorie value in his diet, then insulin was not required. Dr. Graham did not adopt this view, because insulin had some effects other than those on sugar metabolism, and patients felt all the better for it. The patient with the acute attack of diabetes should be given insulin so that the beta cells were not overworked. He referred to Joslin's figures of an average duration of five years for this type of case before the use of insulin, and pointed out that it would be interesting to see if insulin would extend this period. Thirdly, he dealt with the type of diet to be used. The great differences of opinion occurred mainly with regard to the use of carbohydrate. He was in favour of comparatively small quantities and small doses of insulin, the latter being easier for the patient to manipulate and not requiring such exactitude as to time and carbohydrate dosage as if larger doses of insulin and bigger carbohydrate diets were employed. There was also more danger of ill effects after a sudden stoppage if large doses of insulin were used. With regard to the total calorie value, Dr. Graham was in favour of the lower values in the usual figures of 22 to 30 calories per kilo of body weight. Fourthly, with regard to the use of insulin in diabetic coma, the dose should be large—about 100 units a day—and large amounts of sugar should be given at the same time. The influence of intercurrent disease in the production of coma had been strikingly marked in the speaker's experience. Fifthly, Dr. Graham emphasized the influence of other diseases on diabetes. A cold in the head or an attack of diarrhoea in a diabetic would send the blood sugar rapidly up, and it was important to get diabetic patients to realize the seriousness of trivial ailments.

Professor HUGH MACLEAN stated that insulin had been a great success, for by it the majority of uncomplicated cases could be controlled. The best principle was to follow Nature, and while it was ideal to keep blood sugar within normal limits it was often impossible after patients left hospital. He agreed that the presence of a slight amount of the acetone bodies in the urine could be disregarded, but he could not confirm Dr. Graham's fears as to the dangers of suddenly stopping insulin in the case of a patient on a high carbohydrate diet. He found children were the most difficult patients to treat, and described how many developed coma during the winter months. Professor Maclean stated that he had been asked whether a young man suffering from diabetes, controlled with insulin, should marry, and he rather thought the psychological effect of a ban on marriage would produce worse results than marriage might. He thought that insulin should always be used in children, and he almost agreed with the American view that insulin should be used in every patient under 40 suffering from true diabetes. He described an interesting case of diabetic coma and renal glycosuria in the same patient, pointing out the difficulties in treatment. He also had found the tonic effect of small doses of insulin very valuable.

Dr. C. M. WILSON observed that the whole question seemed to him to be still *sub judice*. What were they trying to do? Were they trying to keep the blood sugar normal or the weight steady? He was rather in favour of keeping the blood sugar normal, and was it better to do this by dieting or by using insulin? The question of the tonic effect of insulin very much complicated matters. He inquired as to the effect of insulin treatment in cases of eye complications, which in his experience had been disappointing.

Dr. E. G. B. CALVERT raised the question whether diabetes was a progressive lesion or not. He cited several cases where the diabetes seemed to have got worse even with the blood sugar kept low, and suggested that further investigation was necessary on this subject as to the effect of toxins and other endocrine secretions on the lesion producing diabetes. He described the use of parathyroid extract to enhance the action of insulin, and also quoted some work which suggested that there might be something in the

vitamin content of a diabetic diet which affected blood sugar curves.

Sir JOHN BROADBENT inquired as to the after-results of insulin treatment, quoting a curious case where a patient's tolerance of carbohydrates was very quickly increased with comparatively small doses of insulin; he also referred to the difference in the metabolism of laevulose and dextrose as having some bearing on diet in diabetes.

Dr. W. LANGDON BROWN said that the greatest possible harm had been done by the statement that insulin treatment once begun must be continued for life. The state of a patient after insulin treatment was left off was *not* worse than before. He thought that Dr. Wilson was unduly pessimistic, and stated that cases which came under treatment promptly usually did well. He expressed himself as very interested in the use of parathyroid extract.

Dr. E. P. POULTON dealt with the severe case of diabetes, and expressed his opinion that the large doses of insulin necessary in these cases were better spread out over the twenty-four hours. He thought that, especially in young patients, the earlier insulin treatment was employed the better, and even elderly patients felt great benefit from insulin injections. The form that carbohydrate was taken in was important. He found that roots such as artichokes produced a smaller increase in the blood sugar than greenstuffs containing the same amount of carbohydrate.

Dr. GRAHAM replied to the various speakers. He thought that the question of marriage was of great importance, especially to the female, where the risks of pregnancy were of grave danger to the diabetic. He had not seen any improvement in cases of diabetic retinitis, and he again emphasized the difficulty of treating children, where the danger of various infections added to the fluctuations in the blood sugar.

SEQUELS OF ENCEPHALITIS LETHARGICA.

AT a meeting of the Section of Medicine of the Royal Academy of Medicine in Ireland held on November 21st, the President, Dr. F. C. PURSER, read a paper on the sequelae of encephalitis lethargica.

The President said that the most constant, and perhaps the most distressing, after-effects of encephalitis were mental. Some writers said that in 95 per cent. of cases there were mental sequelae, and many put the figure at 70 per cent.; he was inclined to adopt the lower figure. The type and severity of the initial symptoms determined to a great extent the subsequent mental condition. Patients with marked lethargy and coma were far more prone to mental sequelae than those who were sleepless, delirious, or even maniacal in the acute stage. The young, under 12, were likely to suffer most. Mental deficiency and insanity were frequently recorded as sequelae, including mania, melancholia, dementia praecox, and states of unconsciousness like petit mal, lasting some hours and unaccompanied by convulsions. The immediate outlook in post-encephalitic mania appeared to be good. The milder mental afflictions appeared as impaired memory, a change of disposition, and a disturbance of normal sleep. The alteration in the patient's disposition was always for the worse. They were restless, irritable, and dissatisfied. Most of the physical sequelae were residua of conditions characterizing the acute stage, some form of ocular abnormality being the commonest. This might amount to a total ophthalmoplegia, or be only a slight squint and diplopia; it might be ptosis, unequal pupils, Argyll Robertson pupils, or nystagmus. It was estimated that over 75 per cent. of patients who survived the acute attack showed some ophthalmological peculiarity. In contrast with the age incidence of mental sequelae, children were far less liable to these residua than adults, most cases occurring between the ages of 15 and 40. In general physical residua, with the exception of the Parkinsonian syndrome, tended to abate as time went on. This was especially so with ocular paralyses. There was, however, much reason to fear that physical symptoms which followed an acute attack after an interval of some months, and which were not merely residua persisting from an acute attack, tended to progress rather rapidly. Involuntary movements were said to occur in 15 per cent. of cases, their form varying greatly. Some resembled Sydenham's chorea so closely that no difference could be detected between the two conditions. Hemiplegia, or a less extensive paralysis showing involvement of the pyramidal tract, was not infrequently met with. Sensory residua were indefinite; pains lasting some weeks, and possibly attributable to lesion of a posterior nerve root, might occur. Obesity was an unusual sequel of encephalitis, but it seemed to be the most common trophic change following an acute attack. Finally, the Parkinsonian syndrome was, next to ocular paralysis, the most common sequel of acute encephalitis. It had been computed that about one-quarter of the acute non-fatal cases ended in this tragedy. The post-encephalitic Parkinsonian syndrome differed from typical paralysis agitans in many particulars which, apart from the patient's history of previous illness and other residua of encephalitis which might be present, made the differential diagnosis easy. The differences were etiological and clinical. Whereas paralysis agitans proper commenced after 45, and was commoner in men than in women, the post-encephalitic condition resembling it occurred at any age, from 15 to 40 years usually, and women were at least as frequently affected as men. Further, compared with true paralysis agitans, the condition was steadily and rapidly progressive.

Dr. LEONARD ABRAHAMSON presented two patients with sequelae. The first patient was a boy, aged 11, who had had an attack of encephalitis in March, 1924, with abdominal myoclonus; this myoclonus was still present. It varied in intensity, and the contractions were often more forcible than those noted during the acute attack. In addition to the muscular sequela, the boy showed great irritability, change in disposition, insomnia, and a habit of holding his breath. The second case was that of a girl, aged 19, who had had an attack of encephalitis in April, 1924, characterized by ocular paralysis, insomnia, and delirium. She now suffered from delusions about her food, which she thought was poisoned, and complained of yawning frequently. At times during the yawn her mouth remained open, and she was unable to finish the yawn until some time had elapsed. Another habit developed was that of frequently taking deep breaths. Insomnia was present, and towards evening she showed restlessness of the left foot, which moved continuously during the night. Dr. Abrahamson referred to the variation in the incidence of the sequelae of encephalitis after the different outbreaks. The Parkinsonian syndrome seemed to have been much commoner after the last epidemic than after previous epidemics. He discussed the changes in character noted in children, and referred to a child who developed marked kleptomania. In discussing the frequency of sequelae he referred to the difficulty in compiling accurate statistics, as sequelae were often considerably delayed in appearance, and the patient might seem to have quite recovered from the original illness. In his experience sequelae occurred in about 50 per cent. of patients. In a few cases recrudescences indicated that the virus could remain in the system for long periods.

Dr. J. SPEARES mentioned the difficulty of diagnosing cases of encephalitis lethargica at first sight. He had recently seen three cases of the Parkinsonian type in which the face particularly was affected, and it had been very difficult to get any history of an acute attack of the disease. He had tried hyoscine in these cases, but without much result. The drug he had found most useful was luminal, starting with 1/2-grain doses and increasing to 2-grain doses. A patient of his who had suffered in 1920 from the disease, which was not at first diagnosed, was treated later and got well, but was now practically unable to swallow.

Dr. ALFRED R. PARSONS referred to the frequency of restlessness in cases of sequelae of encephalitis lethargica, both nocturnal and in the daytime. He had at present a patient who was so restless that he could not sit still in a room for five minutes. Occasionally patients recovered quite satisfactorily; he had seen one with complete ocular paralysis who had made a complete recovery, and had no sequelae of any kind whatsoever. He agreed about the difficulty of getting a history of an acute attack from patients. He had recently seen a case, a schoolboy, who had got a blow on the eye; it was thought that the injury

to the eye was quite sufficient to account for his symptoms, but later on he developed well marked Parkinsonian symptoms. In this case there was no history whatever of any acute illness.

Dr. T. G. MOORHEAD referred to a case which he had recently seen, and which displayed in a striking manner the Fröhlich syndrome. The patient, a woman aged 23, employed in clerical work, had suffered from a mild attack of encephalitis lethargica last January. Since then she had been dull and listless, and she now presented a typical Parkinsonian appearance, together with an extraordinary adiposity of the entire body, but more especially the mammary glands and the hips. There was complete amenorrhoea, with an almost complete disappearance of the pubic hair, and there seemed to be little doubt that the condition was due to involvement of the pituitary gland. Dr. Moorhead referred to the possible value of psychotherapy in the nocturnal restlessness of children. In one recent case of his suggestion treatment had proved most valuable after drugs had failed. Of the various drugs used for myoclonic spasm he had found hyoscine by far the most useful, but it was necessary to give it in fairly large doses.

Dr. G. E. NESBITT drew attention to the fact that the Parkinsonian syndrome frequently occurred in the acute stage of encephalitis lethargica as well as in the later stage, and to the frequency of mental deterioration in these cases.

Dr. V. M. SYNGE asked what dose of hyoscine should be given in these cases. He had recently seen a statement by some observers who had obtained excellent results from the use of this drug; some bad cases had got well enough to allow the patients to go back to work, and others had shown a great improvement. The dose given was one-fiftieth of a grain four times daily, and in some cases one-hundredth of a grain four times daily.

The PRESIDENT, in reply, thought the Parkinsonian syndrome occurred in 50 per cent. of cases; in nearly every case the face was affected. He had not found luminal of any use, and hyoscine, even in small doses, had seemed to him to make the patients delirious; although he had found that in cases of paralysis agitans it had stopped the tremor for a time, he had not given it in cases of encephalitis lethargica. He had never got good results from the use of parathyroid extract in any cases, but it had not done any harm. He thought that the Parkinsonian syndrome occurred more frequently in the acute stage of encephalitis lethargica than in the later stages. Urotropine was sometimes given in the acute stages of the disease, and he had heard that intravenous injection of sodium salicylate was very good also in the acute stages, one injection a day being given of 1 gram in 5 c.cm. of normal saline.

THE VALUE OF THE SCHICK TEST.

A MEETING of the Section of Pathology of the Royal Academy of Medicine in Ireland was held on November 7th. The President, Professor J. T. WIGHAM, read a paper on early and pre-morbid diagnosis, with special reference to skin reactions.

Dr. W. M. CROFTON said that apparently the Schick test for diphtheria was not really reliable. It seemed to him much more simple to immunize with toxin itself, or with toxin modified with serum (which he did not think necessary), or modified with iodine, or by inoculations with the diphtheria bacillus itself.

Dr. J. W. BIGGER remarked that it needed a good deal of thought to decide between a positive and a negative Schick reaction. There were some substances which were harmful even to normal people; in some people diphtheria toxin was a harmful thing, and gave a positive reaction. Other substances were harmless; ordinary horse serum when injected into people generally produced no bad result. Certain substances when injected into some people would be harmless, and when injected into others would prove very harmful. He considered that the term "anaphylaxis" should be limited to a condition of sensitiveness produced by the injection of a substance which was usually harmless. There was no doubt about the utility of the Schick test. Some people were very susceptible to diphtheria and others were not, and it would only be necessary to immunize those who had been proved by the test to be susceptible.

Dr. T. T. O'FARRELL raised the question whether the Schick reaction was affected by the virulence of the bacilli. Dr. W. D. O'KELLY did not think it was possible to immunize patients by injecting diphtheria bacilli. If antitoxin was given on one occasion and the patient contracted diphtheria again he was liable to anaphylaxis, though cases of anaphylaxis in man were extremely uncommon.

Dr. E. T. FREEMAN said that the results he had obtained from the Schick test had been very disconcerting. In cases of eczema, etc., in children, one could usually detect some factor in diet which could be incriminated with some degree of certainty. In adults this was usually impossible, and he was of opinion that the results obtained by the test were of very doubtful value.

DIAGNOSIS AND TREATMENT OF GASTRIC AND DUODENAL DISEASE.

AT a meeting of the Nottingham Medico-Chirurgical Society held on November 7th, the President, Dr. W. STAFFORD, presiding, Professor H. MACLEAN of St. Thomas's Hospital gave an address on modern methods in the diagnosis and treatment of gastric and duodenal disease.

Professor MacLean, after a few remarks on the classification of digestive disorders, said that the test meal was of no importance in the diagnosis of gastric or duodenal ulcer, for, apart from the fact that there was generally an increase of hydrochloric acid with duodenal ulcer, test-meal results were normal. In the great majority of cases of cancer of the stomach there was no free hydrochloric acid, but lactic acid was present. These findings *per se* were not pathognomonic of cancer, but if, after the stomach had been thoroughly washed out, a test meal showed the presence of lactic acid, then cancer might be diagnosed. The best solution for testing for lactic acid was ferric chloride 0.2 gram, mercuric chloride 2.5 grams, hydrochloric acid 0.1 c.cm., water to 100 c.cm. A few drops of this solution added to a quarter of an inch or so of filtered test meal in a test tube gave a yellow coloration in the presence of lactic acid. This test was exceedingly useful even in early cases of gastric cancer. The source of the lactic acid in cancer of the stomach was at present unknown. It did not appear to be due to fermentation, but seemed to be produced from the diseased stomach. It was now more or less definitely accepted that gastric ulcer very rarely passed into cancer. In a very few cases it might do so, but as an outside figure not more than 3 to 5 per cent. of gastric ulcers ever gave rise to cancer. In haematemesis from gastric ulcer an operation was contraindicated, since most of the patients died after surgical treatment. It was essential to keep the stomach alkaline, and to give no food by the mouth for several days. Only very small amounts of milk should be given at first, and special care should be taken to keep the stomach alkaline for some time after the food was given. In doubtful cases of ulcer or cancer definite evidence might frequently be obtained by examining the stools for occult blood. When this was found it always signified an abrasion of the alimentary tract in the stomach or duodenum. All the symptoms of ulcer were often present, and yet no ulcer was found at an operation. In such cases a definite band was sometimes found extending from the duodenum to the gall bladder, and when this band was divided the dyspeptic symptoms generally disappeared.

Both duodenal and stomach ulcers should be treated by alkaline therapy, great care being taken that the stomach was really kept alkaline. For this it was necessary to give 10 ounces of milk (containing sodium citrate) every two hours, together with 2 drachms of magnesium cream immediately after the milk. Between the milk feeds 1 to 2 drachms of an alkaline powder, such as sodium bicarbonate, magnesium carbonate, and bismuth carbonate, in equal parts to half an ounce, should be administered. At 10 p.m. or later 1/2 to 1 ounce of bismuth carbonate should be given in water, this treatment to be continued for two weeks.

After that more solid food might gradually be added, but alkaline therapy should be continued for three weeks longer. Careful attention for two years was necessary afterwards. If this treatment failed surgical intervention was indicated. Cases of cholecystitis, gall stones, etc., could often be diagnosed by means of the duodenal tube; suction secured some of the gall-bladder contents, and microscopic examination would frequently reveal pathological changes in the bile.

STREPTOCOCCI IN DISEASE.

AT a meeting of the Manchester Pathological Society held on November 12th, Professor W. W. C. TOPLEY delivered his presidential address on streptococci in relation to human disease.

Professor Topley pointed out that streptococci must be regarded as the chief amongst pathogenic organisms to cause death in man, and particularly in association with affections of the respiratory tract. The classification of these organisms was difficult, and must necessarily be somewhat inaccurate. A classification suitable for ordinary purposes was suggested. The influence of secondary streptococcal infections in influenza and measles was emphasized, and the importance of isolation and careful nursing precautions to prevent the spread of pneumonia in these cases. The mass of clinical and experimental evidence was overwhelmingly in favour of scarlet fever being a primary streptococcal infection; in the case of rheumatic fever, though the evidence was by no means so definite, there was much to support the view that a streptococcus was responsible. The pneumococcus group was also considered, and Professor Topley suggested that the Type I antiserum appeared to have a definite therapeutic value. The certainty as to the efficacy of pneumococcal antiserums could only be arrived at by the careful and combined clinical and bacteriological investigation of a large number of cases.

Dr. COOPER, Professor SHAW DUNN, and Dr. G. J. LANGLEY took part in the subsequent discussion.

UTERINE BACKWARD DISPLACEMENTS.

A MEETING of the North of England Obstetrical and Gynaecological Society was held at Sheffield on November 21st, with the President, Professor A. DONALD, in the chair. The discussion of the President's address on the treatment of mobile retro-displacements, adjourned from the previous meeting of the society (reported in our columns November 8th, p. 858), was resumed. Professor Donald's address is printed in full this week at page 1087.

Mr. W. W. KING (Sheffield) only operated upon those cases of mobile retroversion in which the symptoms were relieved by replacement of the uterus and the use of a pessary. No woman objected to a pessary for three months when it was pointed out that it was being used as a test as to the advisability or otherwise of operation. The use of this test eliminated the necessity for suspension operations in all but a very few cases. He considered that dyspareunia, sterility, and repeated abortions were, of themselves, indications for operation. He related cases of the cure of symptoms by sling suspension operations and their recurrence when the round ligaments gave way, so that the uterus again became retroverted. He took this as proof that retroversion sometimes gave rise to symptoms. He rarely undertook curettage except as a diagnostic procedure or in cases of retained products of conception.

Dr. LEITH MURRAY (Liverpool) had tabulated his cases of ventrosuspension during the three years 1921-23; they numbered 31 out of a total of 593 abdominal operations performed at the Hospital for Women. Of 12 ventrosuspensions, 9 were in married nulliparous women, mainly for sterility; 3 were in single women and were determined by exploration, an original diagnosis of mild inflammatory disease having been made. He agreed that there was little need for suspensory operations in nulliparous women, but contrasted the large number of conditions in which the President had recommended curetting with the rare occasions on which he himself considered this necessary. No single woman had been curetted during the three years analysed, and only one married nulliparous woman. With regard to multiparous women, he found much difficulty in appraising the more usual complications of child-bearing, such as subinvolution, mild disease of the appendages, and slight descent, in relation to symptoms. He considered that in mobile retro-displacements hyperaemia was intrinsic, and was not determined by the backward position. Curettage was the best treatment for subinvolution, except possibly when the uterus was very big, heavy, tender, and retroflexed, when he thought that a suspensory operation in addition to the curettage would more quickly lead to improvement. He preferred the term "functional hyperactivity" to "disordered action of the uterus," and "subinvolution," in most cases, to "endometritis."

Mr. MILES H. PHILLIPS (Sheffield), who could not attend, had asked Dr. JOHN CHISHOLM to express the following views on his behalf: He agreed that the chief symptom of mobile retroversion was pelvic discomfort, varying in amount, and even in the same patient at different times; if neglected, neurasthenia followed often. The other troubles were sterility, liability to abortion, and dyspareunia, more especially when the ovaries were prolapsed. All these symptoms, uncomplicated by other conditions, could be cured by replacement of the uterus, and its retention by a pessary or suspension operation. Mr. Phillips was astonished that cases of this kind occurred so rarely in Dr. Donald's practice. The retroversion, however, was often of much less importance than some other conditions also present—for example, gonococcal or other infection of the cervix, endometrium, myometrium, or appendages; vaginal prolapse; or hypertrophied vaginal cervix. To overlook these and attend only to the retroversion was a frequent cause of disappointment. It was often easy to decide what degree of attention the retroversion required in inflammatory cases, but in others a pessary was first employed as a test before operation was advised. Mr. Phillips was satisfied that retroversion did not cause endometritis. He believed that congestion of the cervix and body of the uterus was produced by retroversion in many but not in all cases, depending on the tone in the broad ligaments and utero-sacral folds. He had on many occasions demonstrated that the engorgement and erosion of the posterior lip of the cervix in retroflexion disappeared after the uterus had been put in its proper position; that must point to some interference with the circulation. Again, drainage was interfered with by retroversion during the puerperium, and occasional cases of lochiometra resulted. In these the benefit of the semi-prone position for an hour or two, or even longer in bad cases, was often seen. Mr. Phillips had seen three cases of acute abdominal pain produced by the retroverted uterus being caught and held by the utero-sacral folds. In two of these cases the surgeon at the operation (abdominal section) had considerable difficulty in dislodging the fundus, although there were no adhesions. In one of these cases the patient was a young nurse who had strained while lifting a heavy bed.

Dr. FLETCHER SHAW, who had on two previous occasions collaborated with Dr. Donald in papers upon this subject, gave the reasons for his agreement with him. Many years ago he had worked under two gynaecologists whose views on this subject differed. In one ward these mobile retroflexed uteri were always fixed up, and in the other ward they were treated by curetting, as an anteflexed uterus with the same symptoms would be; in order to form his own opinion on the subject he had investigated a large number of these cases which had been treated by curetting. The symptoms were tabulated, the position of the uterus noted, and a questionary sent to each patient, asking for the result in each separate symptom. These cases were then divided into two groups: anteflexed and retroflexed uteri. In each group the symptoms were the same, the only difference being the position of the uterus, and so if retroflexion was the cause of the symptoms it would be impossible that curetting, which left the uterus in the same position, should cure this group. The results showed, under every heading,

that the group of retroflexed uteri had a slightly higher percentage of cures than the anteflexed uteri. Quite recently he had made another investigation of a series of his own cases—163 altogether—and found the results exactly the same. The anteflexed uteri gave 75.7 per cent. of cures and the retroflexed 82 per cent. After this investigation it would be impossible to believe otherwise than that a mobile retroflexion did not produce symptoms, except in very rare instances. Most of the speakers who were supporting ventral fixation in this discussion seemed to be arguing largely from these exceptions, since they very seldom performed ventral fixation for simple mobile retroflexion.

PARKINSON'S SYNDROME AND ENCEPHALITIS.

A CLINICAL meeting of the Devon and Exeter Medico-Chirurgical Society was held on November 20th, with the President, Dr. VINCENT SMITH, in the chair.

Dr. W. GORDON showed a man in whom the symptoms of Parkinson's disease had developed after acute encephalitis lethargica. In addition to the symptoms associated with Parkinson's affection, the man had considerable proptosis. Dr. Gordon discussed the pathology of the disease, and traced the relation of the red nucleus (1) with the spinal cord after decussation, (2) and thence via the rubral tract with the anterior cornua, (3) with the corpus striatum and forwards to the frontal convolution. Dr. C. J. VLIELAND compared the pathology in the true Parkinsonian syndrome with that which might have been expected in this particular case. Dr. CRAIG cited foreign cases where Steinach's operation had been undertaken with a view to rejuvenation and arrest of the morbid process. Dr. G. L. THORNTON had recently seen a similar case with proptosis and symptoms of paralysis agitans; a history had been obtained of a preceding illness suggestive of encephalitis.

Dr. GORDON showed, with Dr. MILLER MUIR, a case of lymphadenoma in which *x*-ray applications had caused a considerable reduction in the glandular swelling. Dr. Miller Muir referred to the indirect and distant reduction of glands—in the axilla, for example—when applications were made elsewhere, as in the neck, and also to the diagnostic value of *x* rays, as shown by their effect on the glands in lymphadenoma. Dr. Muir showed also a man with a large inoperable sarcoma of shoulder now under *x*-ray treatment, and of whose ultimate recovery Dr. Muir was hopeful.

Mr. A. L. CANDLER showed photographs of a young woman upon whom he had performed plastic repair of a broken nose. The permanent improvement was very striking, and there was no visible scar, owing to the flap having been turned back from within. Mr. Candler also demonstrated the technique employed in securing rib cartilage for the nasal bridge, and of maintaining free cartilage in reserve in the event of failure of the first attempt. He advocated the fat of the abdominal wall as a useful site for the so-called "human larder."

Dr. R. V. SOLLY showed a specimen of sarcoma of the lung from a man, aged 40, who had been under the care of Dr. F. A. Roper, the clinical diagnosis being made during life. Nearly the whole of the left lung was involved, the growth obliterating the pleural cavity and being strongly adherent to the ribs and spinal column. Enlarged glands were found at the root of the lung and beneath the diaphragm. The heart was pushed over to the right side of the chest, and there was bulging of the left side. The percussion note had been dull over the whole lung, with absence of the breath sounds, tactile fremitus, and auscultatory vocal resonance, except at the extreme apex. Repeated aspiration had been negative on all occasions except once, when a few ounces were withdrawn. The growth proved to be a small round-celled sarcoma.

AT the annual business meeting of the Aberdeen Medico-Chirurgical Society, held on November 20th, the following officers were elected: President, Dr. Peter Howie; vice-president, Dr. James Crombie; secretary, Dr. H. Edgar Smith; treasurer, Dr. George Rose; editor of *Transactions*, Mr. G. H. Colt; librarian, Dr. W. Clark Souter; recording secretary, Dr. H. Ross Souper; assistant secretary, Dr. Robert Richards.

Reviews.

THE EVOLUTION OF CHRONIC PULMONARY TUBERCULOSIS.

PIERRE COLOMBAN, in his book on the evolution of chronic pulmonary tuberculosis,[1] sets out to prove a theory—a very dangerous thing to do, as all who are acquainted with the literature of tuberculosis or cancer will affirm. Briefly, his theory is this: The tubercle bacillus flourishes in tissue which is comparatively avascular; hence when it attacks the adult lung it selects that part where the circulation is poorest—namely, the apex. If the blood flow to this part be increased, the progress of the disease is checked. This increased blood flow can be induced either by drugs which raise the blood pressure, or by placing the patient in a recumbent position with the head at a lower level than the feet, so as to cause passive congestion of the upper parts of the lung.

Neither the circulatory hypothesis of the selective incidence of pulmonary tuberculosis nor the methods of treatment advocated are new, and the sole merit of the author consists in the care he has taken to collect the evidence bearing upon this subject. Some of this evidence is distinctly poor; for instance, he states that in diseases accompanied by hypertension, such as moderate alcoholism, lead poisoning, Bright's disease, and tertiary syphilis, pulmonary tuberculosis, when it does occur, progresses slowly, and he considers this to be due to the increased blood pressure and hence augmented blood flow in the pulmonary circulation which inhibits the activity of the tubercle bacillus. Possibly this is so, but he entirely neglects to observe that these are diseases of middle age—an age, in fact, at which pulmonary tuberculosis naturally pursues a slow course.

It is known that patients with a normal or high blood pressure have a better prognosis than those with a low blood pressure. Not content with observing the systolic and diastolic pressures, the author has invented a method whereby he measures the amplitude of the oscillations of the needle of his Pachon apparatus at gradually diminishing pressures, plots a curve with the pressures along the abscissae and the amplitudes along the ordinates, calculates the area enclosed by the curve, and calls this the "oscillometric surface." This value, expressed in square millimetres, when divided by the product of the patient's weight in kilos and his height in metres, gives the "cardio-vascular coefficient." What this means it is beyond our comprehension to grasp, and, as the author himself says, "its interpretation is delicate." However, in practice he finds that there is a correlation between the size of this coefficient and the prognosis. His main aim is apparently to increase it as much as possible by the method already mentioned. In his opinion the results of this treatment are superior to those obtained by the ordinary hygieno-dietetic treatment.

The book, though marred scientifically by a number of unwarrantable assumptions, has evidently been compiled with great care, and in this respect alone is worthy of the attention of all who are interested in tuberculosis.

DERMATOLOGY.

THE appearance of a second edition of Dr. FREDERICK GARDINER'S *Handbook of Skin Diseases*[2] is proof that it supplies, as the author states in his preface, a felt want. It is remarkable that dermatologists have not recognized the demand among students and practitioners for an abbreviated work of this kind. With the curriculum so extensive the medical student of to-day must often be perplexed to know how to find opportunity for the study of the special subjects in addition to all that is required of him in his general work. For the practitioner probably one of the larger works might prove more attractive, but even for him Dr. Gardiner has provided a useful synopsis of the diagnosis

[1] *L'Evolution de la Tuberculose pulmonaire chronique dans ses rapports avec la circulation générale et locale: La Cure Déclive.* Par Dr. Pierre Colomban (De Grasse). Paris: Masson et Cie. 1924. (Roy. 8vo, pp. xv+228; illustrated. Fr. 12.)

[2] *Handbook of Skin Diseases.* By Frederick Gardiner, M.D., B.Sc. Pub. Health, F.R.C.S.E., F.R.S.E. Second edition. Edinburgh: E. and S. Livingstone. 1924. (Cr. 8vo, pp. 248; 46 figures, 12 plates. 10s. 6d. net.)

and treatment of cutaneous disease. The arrangement of the subject-matter is apparently based upon a series of lectures given in the Edinburgh Medical School, and for this reason differs from the more commonly accepted classification. The many well produced coloured plates give a special character to the work.

The reader will search in vain for a chapter on the subject of eczema—the word does not appear even in the index. The explanation is that Dr. Gardiner shares the view of Sir Norman Walker in preferring the term "dermatitis"; but whether there is any profound advantage in substituting one word, with certain modifications, for another may be a matter of opinion. A well constructed chapter is devoted to the forms of tuberculosis of the skin. Here are described their diagnosis and treatment, and also the comparatively rare manifestations called tuberculides. Evidently this is a subject in which the author takes great interest, and in which he has had considerable experience. Perhaps the syphilitic manifestations of the skin might have been more fully dealt with, for it is the dermatologist who sees the disease in its early and curable stage, and it should be his peculiar function to instruct others in the phenomena belonging to this period.

Taking into consideration the limited space at the author's disposal—and this is a considerable handicap—the result may be said to be good. The book is what it professes to be, a short account of diseases of the skin. Apart from this the standing of the author is a recommendation and should ensure success for the new edition.

A NEW FRENCH TEXTBOOK OF HUMAN ANATOMY.

MOST modern anatomical textbooks have grown to so immense a size that the unfortunate student is bewildered rather than guided and instructed; most teachers are dissatisfied and many are endeavouring to decide the form an ideal treatise on human anatomy for medical students should take. It is therefore a welcome sign to read in Professor H. ROUVIÈRE'S preface to his new *Anatomie Humaine*[3] that he has attempted to remedy two of the most serious and common defects to be noted in the textbooks. In the first place, he has discovered that anatomical literature has been developed on so vast a scale that there is a real risk of obscuring the essential facts and fundamental principles, and of discouraging the reader, if an analysis is made of all the researches published on the various subjects. To eliminate as far as practicable this difficulty, Professor Rouvière has omitted any account of the insignificant details of irregularities and variations, and in selecting references to recent researches claims to have been careful to choose only those which appear to be of greatest importance. It is not to be expected that his selection will command universal approval, and we are bound to say that the omission of a number of highly significant researches, especially some by English and American workers, is unfortunate. In the second place, Professor Rouvière has sought to reduce repetitions inevitable when the body is considered on the more usual system; he has employed the regional method, and with very considerable success.

The book is divided into three principal parts—the first dealing with the head and neck, the second with the trunk, and the third with the extremities; a special chapter is devoted to the central nervous system. The descriptions are in the main accurate and simple, but Professor Rouvière has not quite succeeded in carrying out the intention expressed in the preface of explaining wherever possible the anatomical facts by reference to embryology, comparative anatomy, and physiology. The sections on embryology are clear and adequate, but very slight attention is directed to the intimate relation of structure and function. At a time when so many anatomists are attempting by every available means to learn and teach the "living body," it is disappointing to find in a new textbook accurate accounts of the dead organs, without any reference to the different conditions prevailing during life and the changes they must undergo during normal physiological processes. Unless all teachers of anatomy and writers of anatomical textbooks realize that their aim must be a complete understanding of the structure of the living body, and that structure and function cannot be divorced without serious loss, students will fail to obtain the training and knowledge which anatomy can offer, and progress will be retarded. The information which can only be gained by personal dissection of the dead body must be corrected and supplemented during oral teaching and in textbooks by frequent references to the conditions in the living; in no other way can a real interest in anatomy be aroused and the student receive an efficient training for his future work. In the chapter on the nervous system the more important pathways and connexions have been selected, but the references to their function are quite inadequate, and an unfortunate confusion occurs between the spino-cerebellar tracts and the paths for the conduction of impulses induced by painful and thermal stimuli.

The two volumes are printed on much thicker paper than any other modern anatomical textbook we have seen, and this at first sight obscures the reduction in the length of the text which is undoubtedly one of the book's most striking features. It is freely illustrated, nearly one thousand excellent figures being included, many of which are coloured, and a high proportion prepared from special dissections made by the author. Each volume is provided with an adequate index.

[3] *Anatomie Humaine: Descriptive et Topographique.* Par H. Rouvière. Tome I, Tête, Cou et Tronc; Tome II, Membres, Système Nerveux Central. Paris: Masson et Cie. 1924. (Cr. 4to. Tome I, pp. xiii+1011, 520 figures; Tome II, pp. ix + 664, 468 figures. Paper, Fr. 165; bound, Fr. 180.)

"EUBIOTICS."

L'Eubiotique Sociale is the title of the French edition of a Tcheco-Slovakian treatise[4] by Dr. Růžička, professor of hygiene and dean of the faculty of medicine in the Tcheco-Slovakian University of Bratislava. Eubiotics may be accepted as its English equivalent, and may be defined as the science of living a healthy life just as eugenics may be defined as the science of producing a healthy life. Dr. Růžička discusses the measures required to establish a healthy organization of social life in natural surroundings. A life in touch with Nature is the dominant note throughout. He considers that the only rational basis of civilization is to bring the family into permanent touch with Nature by living in more or less country surroundings. Present-day civilization, he tells us, has lost sight of this essential factor in the physical and moral health of human beings, because the evolution of a civilization based on industrial life involving urban in place of rural dwellings is opposed to the physiological and spiritual needs of the natural life of a man and his family. Such a civilization has produced a deformed kind of existence which cities, as they now are, perpetuate.

His remedy is to remodel the cities and make the buildings in them merely centres of business and public offices, to which the workers in them will be conveyed from rural dwellings during the day and from which they will return in the evening to enjoy a peaceful and healthy family life in their own cottages, gardens, and farmyards. To emphasize his point he contrasts in parallel columns the life of a family in the country with that of a family in the town. Having drawn this picture of the physical and moral evils of a city home and the joys of a rural existence, Dr. Růžička proceeds to discuss the practical aspects of eubiotics. He estimates that, on a basis of 2,000 square metres, approximately an acre, being the requirement for a country cottage for a family of five, one square kilometre would be sufficient for a garden city of 500 families; and, as the area of Tcheco-Slovakia is 141,000 square kilometres and the population 3,000,000, only 6,000 square kilometres of the total area would be needed to house the whole of the population in rural surroundings, or, as he continues to express it, "in constant touch with Nature." The chief difficulty, he foresees, would be the provision of cheap and rapid means of transport from the garden city areas to the centres of business and factories. A garden city, for

[4] *L'Eubiotique Sociale.* By Dr. Stan. Růžička. Paris: A. Maloine et Fils. 1923. (Cr. 4to, pp. 52; 1 illustration. Fr. 8.50.)

example, in circular form would be thirteen miles in diameter for a population of 1,000,000 in families averaging five persons each; and this would require an extensive network of tramways or other cheap means of transport to and from the work centres. Dr. Růžička, however, sees no obstacles to the realization of such a scheme in Tcheco-Slovakia, where the problem of cheap transport is likely to be solved in time by electric power. Its Government has already voted a sum of 75,000,000 crowns as a preliminary contribution towards the eventual electrification of the whole of the State. Dr. Růžička regards this as the most essential factor in the remodelling of cities by the removal of the inhabitants to areas of garden cities at a distance from their centres of work. But what is possible in one country may not be so in a country where Nature does not provide cheap methods of producing electric power.

The acquisition of a *rus in urbe*, on which a life that is, according to Dr. Růžička, eubiotic depends, is one of the city dweller's oldest aspirations. The garden cities of Letchworth and Welwyn are pioneer attempts at eubiotics in our own country, but they are still far short of Dr. Růžička's Utopia. Eubiotics is no more an exact science than eugenics, and neither is likely to be fully realized in this world of troubles. *L'Eubiotique Sociale* is nevertheless an interesting study of a question of physical and moral hygiene. It is unfortunately marred by constant repetitions and emotional emphasis of words, lines, sentences, and headings by single and double underlining, capitals, italics, and suchlike devices for attracting attention.

GUY'S HOSPITAL REPORTS.

THE last quarterly instalment of the seventy-fourth volume of the *Guy's Hospital Reports*[5] contains ten papers, of which two deal with the alimentary canal, two with oral infections, and two with endocrine affections. The editor, Dr. ARTHUR F. HURST, contributes an interesting review of the pathogenesis of gastric and duodenal ulcers, dealing with the influence of acid, the infective factor, the predisposing factors—namely, the ulcer diathesis—and with the accessory factors, such as excessive smoking, mental and physical fatigue and suprarenal exhaustion, and food. In explanation of the mystery why chronic gastric ulcers are prone to become malignant, whereas chronic duodenal ulcers are not, he suggests that it is to be explained by the constant mechanical irritation to which gastric, but not duodenal, ulcers are exposed. In collaboration with Mr. P. J. Briggs, he also writes on diverticula of the stomach as a possible source of error in the radiological diagnosis of gastric ulcer, and records two cases, with ten radiograms in illustration.

In a short paper Dr. J. Eyre shows that alveolar osteitis due to infection by *Streptococcus pyogenes longus* (var. *viridans*) may persist and progress for months after the teeth implicated have been extracted. From an experimental study of *Leptothrix buccalis* Mr. Arthur Bulleid concludes that there is some reason to believe that this fungus plays an active part in the production of tartar, but that for its full and unrestricted activity some accessory factors, such as suitable protection, special pabulum, and possibly some particular microbic symbiosis, are necessary. From the analysis by Dr. J. J. Conybeare and G. C. Millis of twenty-nine cases of Addison's disease in Guy's Hospital between 1904 and 1923, it appears that while the twenty-two cases due to tuberculous change ran a rapid course, the seven cases with simple atrophy survived for much longer periods—one for ten years, one for five years, and two for more than a year. There was very little evidence that adrenaline had any beneficial effect; and two patients on whom grafting of adrenals was performed died rapidly. Dr. C. P. Symonds and Mr. N. L. Eckhoff describe a case of Fröhlich's syndrome associated with a cystic tumour involving both optic thalami, and situated above but apparently not invading the pituitary; to this article is appended a commentary on the functions commonly ascribed to the pituitary. Mr. A. W. Ormond and Mr. R. G. Williams publish a case of arachno-dactyly with special reference to the ocular symptoms, particularly congenital dislocation of the lens with concomitant trembling irides, which were reported in nine out of the seventeen cases they have collected. In reference to the pathogenesis, arguments are brought forward against the view that it is due to endocrine disorder. Dr. N. L. Lloyd writes on methaemoglobinaemia, Dr. W. L. Watt on deep *x*-ray therapy, and the last article is the second series of "Some cases from 'Clinical'" during the first quarter of this year.

[5] *Guy's Hospital Reports*, Vol. lxxiv (Vol. iv. Fourth Series), No. 4, October, 1924. Edited by A. F. Hurst, M.D. London: Wakley and Son (1912), Ltd. 1924. (Med. 8vo, pp. 369+490; 49 figures. Subscription £2 2s. for volume of four parts; single numbers 12s. 6d. each.)

WAR AGAINST RATS.

MR. MARK HOVELL'S book, *Rats and How to Destroy Them*,[6] gives in summary form the record of forty years' relentless war waged by him against the rat tribe. His general conclusions and deductions are the outcome of applied scientific research and common sense. His explanations and instructions are clear and readable. The mechanical details of trapping and "blocking," apart from the lucid letterpress, are admirably illustrated in all necessary detail. The manual is specially arranged so that each phase of rat destruction and prevention is treated as a whole, thereby enabling a reader who desires to get rid of rats on his premises to turn up the particular chapter without having to read the whole book. Urban and rural conditions vary. Consequently, Mr. Hovell has chapters dealing separately with houses, shops, and warehouses, outbuildings, yards, pigsties, and other buildings, and also with gardens, streams, sewers, sporting estates, farms, and ships.

Whilst the recurrence of individual paragraphs and illustrations would, in certain circumstances, mar the readable qualities of the work, the primary object—namely, to enable people to deal with the pest promptly, without unnecessary waste of time—outweighs other considerations.

The book demonstrates by tables the enormous loss to the country of foodstuffs due to rats, and deals with the diseases caused and conveyed by rats; a section devoted to plague gives an interesting account of facts connected with that disease.

Every medical officer of health, and every estate agent too, should have a copy in his reference library. The reviewer has been a rat destroyer from his tenderest years, and, though pets were forbidden, kept ferrets at school, thus becoming a cause of offence to his schoolfellows when he took the said ferrets in his "tails" to chapel, etc.; he has, for all that, discovered many fresh wrinkles in this book, very few points for criticism, and these trivial. One has reference to the fecundity of the rat. Mr. Hovell states that one pair and their descendants in twelve months produce 1,130. Though in making this calculation he has been exceedingly cautious and conservative, he makes no allowance for casualties, in the shape of battle, murder, and sudden death, which, even without human agency, fortunately occur. The second point is equally slight. The question of cost is ever present in all vermin destruction. Red squill appears to be one of the safest and best of poisons. If the gardener and farmer knew he could grow the bulbs of *Scilla maritima* and how to prepare the dried bulb for poisoning purposes it would perhaps be an advantage. For the rest Mr. BENSUSAN'S introduction is, in itself, a sufficient guarantee of the excellence of the book.

CUTHBERT JAMES.

NOTES ON BOOKS.

THE sixth edition of Mr. L. BATHE RAWLING'S *Landmarks and Surface Markings of the Human Body*[7] contains several new illustrations and a certain amount of revision of the text. The fifth edition, which was reviewed in our columns of July 19th, 1913 (p. 127), has been reprinted seven times, and the demand for this book has been deservedly great. The illustrations, which are so valuable a feature of the work, are even clearer than before, and the use of colour in depicting

[6] *Rats and How to Destroy Them.* By Mark Hovell, F.R.C.S. With an introduction by S. L. Bensusan. London: John Bale, Sons, and Danielsson, Ltd. 1924. (Demy 8vo, pp. xlii + 465; 51 figures. 10s. 6d. net.; post free, 11s. 3d. inland, 12s. abroad.)

[7] *Landmarks and Surface Markings of the Human Body.* By L. Bathe Rawling, M.B., B.C.Cantab., F.R.C.S. Sixth edition. London: H. K. Lewis and Co., Ltd. 1924. (Demy 8vo, pp. viii + 97; 36 figures. 7s. 6d.)

details facilitates considerably the memorizing of anatomical relationships by the student. This edition will doubtless be as popular as its predecessors, both with students and surgeons.

In *Economics of Fatigue and Unrest and the Efficiency of Labour in English and American Industry*,[8] Dr. P. SARGANT FLORENCE has given a very comprehensive and readable study of the underlying causes of these important industrial conditions. He has collected information and statistics from many sources in Great Britain, Europe, and the United States, which show that definite psychological and physiological causes are concerned. He discusses the methods of preventing and curing these conditions, and lays stress on the human factor in industry. This book will probably interest more especially those engaged in health and welfare work, but in the present critical state of industry it will be appreciated by others less immediately concerned.

Mr. BERESFORD CHANCELLOR has undertaken to write *The Lives of the Rakes* in five volumes, and the first two,[9] dealing with the Restoration period, have just been issued. The *New English Dictionary* defines the word "rake" in this sense as "a man of loose habits and immoral character; an idle, dissipated man of fashion," and says that it is an abbreviation of rake-hell, a word, it states, which passed out of use about 1725. To afford subject for a biography a century or two later a man must have been something more than merely "a dissipated man of fashion"; he must have been a wit or a poet, or both, and Mr. Chancellor's selections fulfil this condition. The first volume, *Old Rowley* (King Charles II), is practically a biography of that monarch, omitting politics after the Restoration. Why he was called "Old Rowley" is not very clear; it was probably not meant to be complimentary, and "Old Rowley, the King" was the title of a popular ballad of the day. Undoubtedly Charles was witty, and the rakes of his Court, who form the subject of the second volume, were all wits too and some of them poets also, including Buckingham, Rochester, and Sedley. Many of the pranks they played to the amusement of Charles were rather silly practical jokes, and a few of them grossly indecent, but in mitigation it may be pleaded that they had not the same wide choice of amusements as the man of fashion to-day; they played tennis sometimes and did a little horse-racing at Epsom and Newmarket, but the roads were bad and the coaches heavy, so that there was difficulty in getting about, and for the most part probably they led a more or less sedentary life hanging about the Court. Pepys notes as early as 1660, "Court attendance infinite tedious." Mr. Chancellor, however, has taken care not to be tedious. His volumes are amusing, and both of them well illustrated with portraits. In view of the season it may be said that the first volume stands more or less by itself.

In noticing the issue of the *Medical Directory* for 1925 in our last issue (p. 1059) we observed that the omission of the directory of dentists would be rather inconvenient to the medical profession, even though the publishers intended to publish a separate directory. We must confess, however, after having seen the *Dental Surgeons' Directory, 1925*,[10] that inclusion of dentists would have added very seriously to the bulk of the *Medical Directory*. The new volume contains 250 pages, and comprises particulars, the main part furnished by themselves, of British licentiates and graduates in dental surgery practising in Great Britain, Scotland, Ireland, and abroad. There is a local list, and also a list of examining bodies and dental schools and hospitals.

Dr. W. S. DAWSON, who is senior assistant medical officer at the Maudsley Hospital, has written a small volume entitled *Aids to Psychiatry*,[11] which will be found useful by those who are taking up the study of mental disorder or who are preparing for examination in this subject. The book includes in a small compass the subject-matter of the ordinary text-book, and it is thoroughly up to date. The author provides a concise description of the various forms of mental disorder and devotes chapters to normal psychology, psychopathology, the causation of mental disorders, their treatment and differential diagnosis, theories of the neuroses, legal aspects of insanity, and case-taking. The book is readable and written with commendable clearness.

[8] *Economics of Fatigue and Unrest and the Efficiency of Labour in English and American Industry.* By P. Sargant Florence, Ph.D. London: G. Allen and Unwin, Ltd. 1924. (Demy 8vo, pp. 426. 16s. net.)

[9] *The Lives of the Rakes.* Vol. I: *Old Rowley* (Charles II). Vol. II: *The Restoration Rakes.* Philip Allan and Co. 1924. (Medium 8vo. 10s. 6d. each.)

[10] *The Dental Surgeons' Directory, 1925.* Fifty-eighth annual issue. (Formerly part of the *Medical Directory.*) London: J. and A. Churchill. (8s. 6d. net.)

[11] *Aids to Psychiatry.* By W. S. Dawson, M.A., M.D.Oxon., M.R.C.P. Lond., D.P.M. London: Baillière, Tindall and Cox. 1924. (Fcap. 8vo, pp. viii + 309. 4s. 6d. net.)

MEDICAL AND SURGICAL APPLIANCES.

A Folding Retinoscope.

DR. G. ZACHARIAH (Egmore, Madras) writes: The retinoscope shown in the accompanying sketch consists of two mirrors, a plane and a concave, fitted at either end of a foldable vulcanite handle. The radius of curvature of the concave mirror is 16 cm. for working at arm's length. There is no hole through the mirrors, but the silvering is scratched off for an area of 2 mm., giving a clear opening through glass; this practically eliminates the disturbing shadow one gets in the ordinary form with a hole. The presence of two mirrors in one handle enables one to use either the plane or the concave at will, and to check the results of the one with the other at once. The folding arrangement, suggested by the makers, makes it a very convenient form to be carried about in the pocket without the mirrors being scratched. Messrs. Dixey and Son made this model for me three years ago, and I find it extremely useful in my work.

ROYAL MEDICAL BENEVOLENT FUND.

AT the last meeting of the Committee thirty-seven cases were considered and £555 voted to thirty-three applicants. The following is a summary of some of the cases relieved.

Widow, aged 70, of F.R.C.P.Ed. recently deceased, who was in receipt of £2 3s. 4d. a month from the Fund on giving up his appointment owing to ill health. The Royal College of Physicians granted £25 towards funeral expenses, and friends gave £10. Applicant is left without means, and the Fund voted her a special grant of £10, and £25 in twelve instalments.

Widow, aged 58, of M.D. who died in April last. Applicant has let her flat furnished and is staying with her married daughter, whose circumstances do not permit her to help her mother to any great extent. Voted £10.

Widow, aged 63, of L.R.C.P.Ed. who died in 1915. Applicant has been able to help herself up to the present by taking various posts, but owing to an accident she is now lame. Her only income is the eighth share of some property, amounting to £40 per annum, but this is not always regularly paid. Her rent is 30s. a week. Voted £25 in twelve instalments.

Widow, aged 51, of M.D.Durh. who died in 1899. Applicant has paralysis in legs and feet; she has tried to support herself by teaching music, but earns on an average only 7s. 6d. a week. The case was referred to the Fund by the St. Andrew's Society, which allows her £39 and gave a grant of £5 to help her to canvass for an annuity. Voted £18 in twelve instalments.

Daughter, aged 50, of M.R.C.S.Eng. who died in 1895. Applicant suffers from tabes dorsalis and finds it hard to support herself. When able she makes sweets and cakes. Her only income apart from this is an annuity of £40 from the Army Officers' Benevolent Fund. Voted £18 in twelve instalments.

Subscriptions may be sent to the Honorary Treasurer, Sir Charters Symonds, K.B.E., C.B., M.S., at 11, Chandos Street, Cavendish Square, London, W.1.

The Royal Medical Benevolent Fund Guild still receives many applications for clothing, especially for coats and skirts for ladies and girls holding secretarial posts, and suits for working boys. The Guild appeals for second-hand clothes and household articles. The gifts should be sent to the Secretary of the Guild, 58, Great Marlborough Street, W.1.

ROYAL COMMISSION ON LUNACY AND MENTAL DISORDER.

(*Continued from page 1012.*)

THE Royal Commission on Lunacy and Mental Disorder resumed its sittings on December 2nd, under the chairmanship of the Right Hon. H. P. MACMILLAN, K.C.

The Administration of London Mental Hospitals.

Mr. H. F. KEENE, chief officer, Mental Hospitals Department, London County Council, continued his evidence. He was questioned as to the new dietary arrangements which were instituted in London mental hospitals in 1923 and which superseded a dietary scale based on a *per capita* allowance. Discrimination was now made in the matter of diet between manual and non-manual workers; plenty of green food was provided, and the dietary was altogether less rigid than under the old system. The arrangement, although instituted for an experimental period of six months, was being continued. During 1923 the number of patients admitted to London mental hospitals was 3,152, and 881 persons were discharged as recovered. In the twenty-nine years since 1895, when statistics began to be kept, 30,718 patients had been discharged recovered, and 9,177 of these (29.87 per cent.) had been readmitted. About one-third of this latter number had been readmitted within twelve months of discharge. An unknown number of the other patients might, of course, have been readmitted to other institutions outside the county of London. In the nine London mental hospitals on January 1st, 1924, there were 18,918 patients (7,728 males and 11,190 females); of these, 1,063 were private list patients, and 120 were women patients admitted as private patients to the special wards provided at Horton Mental Hospital. Of the 3,152 who were admitted during 1923, 2,978 were admitted as patients in receipt of relief, 128 were lunatics at large, 39 were admitted because they were not under proper care or were cruelly treated or neglected, and 7 were admitted as private patients on petition.

The witness was questioned as to how far Visiting Committees safeguarded the interests of the patients, the Chairman remarking

that it seemed to him that the Visiting Committee was the patients' safeguard as distinguished from the staff of the institution, and he was anxious to see that no obstacles were interposed between the patient and the Visiting Committee. He felt also that certain discriminations were made against the rate-aided patient, and the Commission had had a good deal of evidence to show that the position of private and of rate-aided patients ought to be assimilated as regards their legal rights, more especially as regards the procedure in dealing with complaints. Mr. Keene said that he thought the present arrangements with regard to Visiting Committees did safeguard the interest of the patients, and that the patients were further protected by the medical officers and by the chaplain and visiting ministers.

Finally, the witness dealt with the position of the Metropolitan Asylums Board in relation to mental cases. While the population of the mental hospitals under the London County Council was 18,918, that of the institutions of the Metropolitan Asylums Board was 5,148. The former population had largely increased during recent years, but there had been actually a diminution in the number of chronic and harmless lunatics in the Metropolitan Asylums Board hospitals. This was partly due to the operation of the Mental Deficiency Act, but it illustrated the difficulties which arose when two authorities were dealing with the same class of persons.

The Functions of Visiting Committees.

Mr. W. G. LOBJOIT, J.P., chairman of the Visiting Committee of the Middlesex Mental Hospital, strongly urged that two certificates should be requisite for a detention order in pauper as well as in private cases. The Chairman said that he agreed that the two classes of patients should be assimilated, but it was a question whether the assimilation should not take the form of requiring only one certificate in each case, instead of two as at present in the case of a private patient. The Commission did not want to recommend otiose safeguards, or safeguards designed to allay the apprehension of risks which did not actually exist. What he desired to know was whether there was a genuine risk which would be safeguarded against by the requirement of two certificates in the case of a pauper patient. Mr. Lobjoit was of opinion that if recourse was had to only one certificate that certificate should be given by a doctor qualified in mental diseases. The Chairman rejoined that, as the majority of cases presented no difficulty, it would perhaps serve the purpose if in any case of doubt the medical practitioner or the justice was entitled to call in the service of an expert to supplement the medical testimony already given.

Mr. W. E. LOVSEY, chairman of the Visiting Committee of the Birmingham Mental Hospitals, agreed that it was desirable that in every respect the procedure in the case of the Poor Law patient should be identical with that in the case of the private patient. He was examined on the subject of discharges, and said that in a few cases in his experience—not more than fifteen cases during the last ten years—patients had been discharged on the initiative of the Visiting Committee without the full approval of the medical superintendent. In one case only, so far as he knew, was there any disaster. The patient was known personally to certain members of the Visiting Committee, and was discharged, although the medical superintendent was averse, and shortly after his discharge the man committed suicide. Mr. JOWITT pressed the witness as to whether, apart from the very few cases discharged on the initiative of Visiting Committees, a patient once certified had any chance of getting his discharge apart from the approval of the medical superintendent, who, naturally, knowing that he incurred a certain responsibility in discharging a patient, might hesitate to take the step after it became advisable. The witness agreed that in theory this might be so, but in practice he thought the present system of discharge worked without any injustice. With regard to allegations of ill treatment, the witness said that in his ten years' experience disciplinary action had had to be taken in only one case, that of a probationer. He considered that the criticism of asylums was largely the work of busybodies in every locality. The Chairman agreed that the administration of lunacy laws was peculiarly subject to sinister suspicions. Earl RUSSELL suggested that the remedy was publicity, and that it would be very desirable if lay persons like the witness could give popular lectures on asylum administration.

Further Evidence of Medical Superintendents.

The first witness on December 3rd was Dr Henry Yellowlees, O.B.E., M.D., medical superintendent of The Retreat, York, who was accompanied by Mrs. Crosland, a member of the managing committee. The Chairman, the Right Hon. H. P. MACMILLAN, K.C., remarked upon the pleasure of seeing in the witness chair the son of a distinguished doctor who was much honoured in Scotland.

Dr. YELLOWLEES said that he had been medical superintendent of The Retreat since early in 1922. The institution, which dealt only with private cases, was founded by William Tuke, of the Society of Friends, in 1792. It really stood at the commencement of the humane treatment of the insane. In reply to Sir Humphry Rolleston, who asked whether the witness would claim that Tuke was earlier than Pinel in the Salpêtrière and the Bicêtre, the witness said that both men appeared to have started their work in the same year—1792—but there was a suggestion that they were both anticipated by some reformer in Italy. The Retreat was run on lines similar to those of the other twelve registered hospitals in this country—that is to say, by a voluntary committee which worked without payment and had no financial interest in the institution. It was an old-fashioned building, with modern extensions. Although it had two or three associated villas it was not on the villa principle. The beds numbered 180. It was mainly self-supporting, with a few endowed funds. The medical staff ordinarily consisted of two doctors—one man and one woman—in addition to himself. He was quite emphatic upon the value of a woman doctor. He then gave details of the nursing staff and the facilities for the training of nurses. He went on to describe the visits of members of the Board of Control; very few patients took advantage of such visits to lodge complaints. Permission was given to patients to go on periodic shopping expeditions, not only as a species of recreation, but as a means of testing their capacity to mix with ordinary people. This was also helped by concerts, and lectures, and games. If patients with musical tastes were making progress, they were allowed to attend a concert in the town. He and his colleagues in the institution drew up a questionary to ascertain from patients whether they resented their detention. Out of 150 certified patients, 15 replied that they resented their detention, 89 replied that they did not, and 46 were incoherent or doubtful or silent. With regard to discharges, when a patient was approaching recovery he informed the relatives, but in some cases, when the homes were unsatisfactory, there was some hesitation about having the patient back. When poverty could not be alleged, and it was apparent there was some other motive, he brought about the discharge and compelled the friends to do their part.

With regard to the provision in his institution for treatment Dr. Yellowlees said that the psychological treatment, including analysis (if thought desirable) or suggestion, was given, and glandular therapy and any other approved means was used. He had never used, nor could he justify, any treatment such as croton oil being given as a punishment. Hypnotic and similar drugs were carefully served out under medical supervision, and were in the charge of a sister. The treatment might be said to include recreations and concerts. If a patient suffered from sleeplessness he lived on the verandah all day. The treatment of the patient from the medical standpoint was decided upon after a careful physical and mental examination.

In answer to further questions, Dr. Yellowlees said that the nurses employed in his institution were in the proportion of one to rather less than two patients. It was a high proportion, and in a sense the figure was misleading, because a large number of patients paid for the exclusive services of one or perhaps two nurses. The standard charge at the institution was five guineas weekly, but higher rates were charged for private accommodation and for special nursing, and specially low rates for persons connected with the Society of Friends. The charges resulted in a small profit to the institution, which was applied to improvements or to reducing the fees. The cases admitted were not selected, except that patients suffering from epilepsy or from the grosser forms of congenital mental defect were not taken. Many general paralysis cases were admitted. He went on to speak of the difficulty of grading patients. Not the least of the difficulties arose from the fact that while many hopeless cases were quite suitable for association with newly admitted patients or any others, many of the recoverable cases were unpleasant and possibly dangerous. Thus an admission block confined purely to recent cases was not so easy a proposition as it sounded. Of four consecutive admissions, each patient admitted might be quite unsuitable for mixing with the other three. Rather more than half the admissions in one year were voluntary boarders, and not certifiable. If he thought a case was going to recover he used the law's delays as far as he safely could, in the interests of the patient, to avoid certification. Asked whether there was a possibility that the medical superintendent, through fear of incurring responsibility, might detain patients too long, the witness said that he thought the danger was in the other direction; the superintendent was more likely to let out a patient too soon, not that the patient thus prematurely discharged was likely to be a danger to himself or anyone else, but that his cure might have been more complete if he had had a longer stay. The immense majority of the cases in which there was any real difficulty about discharge were the convalescent melancholics. Sir David Drummond questioned the witness about the advantages of having a lady doctor on the staff of a mental institution. The witness said that the advantage was obvious, because more than half the patients were women, and some of them had expressed a preference for a lady doctor. Sir D. Drummond asked whether the witness would suggest that the medical superintendent should be a lady, and the witness caused laughter by his reply, "Not yet." He added that he did not think that men had any prescriptive right to be asylum superintendents.

Dr. J. G. PORTER PHILLIPS, physician-superintendent of Bethlem Royal Hospital, was the next witness, and was accompanied by Mr. Lionel Faudel Phillips, governor of that institution. Dr. Porter Phillips, after giving details of his own experience in dealing with mental cases, said that the voluntary boarder system had been in vogue at Bethlem for forty years, and he believed it to be a system deserving of encouragement in many ways. Almost 40 per cent. of the patients were voluntary inmates. The straightforward voluntary patient gave no trouble; he knew he was ill, and he welcomed the advice and treatment he received. Such voluntary patients were not necessarily paying patients; those admitted on a free basis represented sometimes 70 per cent. of all the voluntary admissions. The recovery rate among the voluntary patients was about 50 per cent. He was in favour of a temporary observation order, which would involve some power of control, short of certification. His experience with his clinic at St. Bartholomew's showed that a certain number of mental cases could be adequately treated in a voluntary hospital, where they would have no anxiety about detention, nor any stigma. The drawback was that in the intervals between coming up for treatment they tended to deteriorate owing to the pressure of home cares and conditions.

Mr. FAUDEL PHILLIPS spoke of his method of interviewing patients and investigating complaints; in twenty years he had known no complaints on the score of treatment.

British Medical Journal.

SATURDAY, DECEMBER 13TH, 1924.

MEDICINE IN INDIA.

THE Cabinet decision to accept certain recommendations made last March by the Lee Commission on the Superior Civil Services in India, in respect of improvements in pay and pensions and the grant of free passages, will be received with general satisfaction, in this country at least. The decision concedes the claim of those whom it affects to a fair economic standard of life, not only as the due reward of service rendered, but as one means of making such service effectual. It would be rash to assume that it is more than this, or that it will of itself suffice to secure an adequate flow of recruits from the universities to the Indian services. In this respect Lord Sydenham's warning in the *Times* of December 8th is opportune. The Commission itself fully recognized that its main proposals were "vitally interdependent," and urged that "they should be considered as a whole and acted on with as little delay as possible." Comparison of these proposals with the official communiqué shows that the latter is silent as to the very issues which are at once most complex and most fundamental to the development of the Indian commonwealth. Among these is the reorganization of medical services in India. In this connexion the wisdom of the expedients recommended is open to the gravest doubt. The proposed provincialization of the superior medical services is calculated to increase rather than to remedy the existing difficulty in obtaining for India the services of the quota of British practitioners requisite during the process of Indianization; and from this it must follow that the British residents will fail to secure medical attendance by British practitioners, that the military reserve of British qualified practitioners necessary in time of war will not be available, and that scientific medicine in India will suffer the loss of a due proportion of investigators trained in European schools.

On this aspect of the matter the Council of the British Medical Association has addressed to the Secretary of State for India representations which will doubtless receive full consideration. For the moment we have no indication of the attitude either of the Secretary of State or of the Government of India, and detailed discussion would be out of place. The point we would emphasize, and the Government will do well to bear in mind, is that the basic question at issue is wider than the adjustment of conflicting service interests or the construction of an administrative machine which will perform correctly certain limited functions. Rather it is the provision for making available for the population of India, agricultural as well as urban, the resources of modern scientific medicine, and at the same time for creating standards of medical education and medical practice which will enable Indian practitioners to make in the future a worthy contribution to the development of medical science. We would refer those who wish to understand the difficulties of the position from an administrative point of view to the paper on the future of medicine in India read by Sir Gerald Giffard to the East India Association, and reported in the JOURNAL of November 22nd (p. 966). The conditions shown by Sir Norman Walker's report on the teaching of midwifery in the Indian medical schools, the subsequent failure of Calcutta University to maintain the standard required by the General Medical Council for the admission of its degrees to the British *Medical Register*, and the tendency to divert the slender financial resources so urgently required for other purposes to the resuscitation of the moribund Ayurvedic and Unani systems of medicine, are all factors in the problem, and all point to the imperative need for strengthening rather than weakening the link between Indian and Western schools of medicine.

Meanwhile, the effect of the accepted concessions on the remaining members of the I.M.S. may be briefly summarized as follows: Captains with more than three years' service will be entitled to Rs. 250 a month overseas pay, as against the present rate of Rs. 200; whilst majors with not less than eleven years' service and lieutenant-colonels will receive Rs. 300 in place of Rs. 250. Such pay may be remitted to this country at a fixed rate of 2s., the present rupee exchange tending to remain steady at 1s. 6d. Four return passages to England will be allowed to officers and their wives in the course of their service, and a single passage for each child. Officers holding administrative appointments of a value not exceeding Rs. 3,000 a month are to receive a monthly sterling addition of £13 6s. 8d., subject to a review by the Government of India of the posts to be covered by this provision. These increases are to be treated on the same basis as pay—that is to say, they will not be dependent upon the recurring votes of the Indian Legislatures, and they will be made effective as from April 1st last.

PREVENTION OF BOVINE TUBERCULOSIS.

VACCINATION against tuberculosis has so often been attempted, and has as often led to disappointment, that developments of the tuberculosis campaign on this line are apt to be looked on with more than a little suspicion. However, Dr. Guérin of the Pasteur Institute of Lille[1] believes that ultimate success is not only certain, but that it is within measurable distance of being realized. Hitherto attempts to minimize the enormous losses in cattle caused yearly by the tubercle bacillus have been practised mainly by some method of isolation and segregation. It is not always understood how important this disease is to the stock-breeder. It may be taken that on an average 18 per cent. of all cattle are infected—the percentage being naturally higher in confined animals than in those which live a free open life. Tuberculous carcasses are seized by the meat inspection authorities, and infected animals cost much more to fatten than normal. Moreover, the lesions are frequently situated in the mammae, and this not only causes contamination of the milk, but as a rule results in a much reduced yield. In cattle the tuberculin test is the usual criterion of infection; but a positive reaction does not necessarily mean that the subject presents grave lesions or can propagate the disease. Moreover, it cannot be declared that non-reactors do not harbour the bacillus in a dormant condition. The attempt to explain why this dormant condition exists, and why such subjects do not rapidly succumb to tuberculosis, has been the subject of much research in recent times.

It is, however, over twenty-five years since Marfan produced evidence that children with tuberculous glands only very exceptionally become consumptive when adult. This implies, of course, that children

[1] *Réc. Méd. Vét.*, 1924, C, pp. 577-582.

who retain a mild dormant form of the disease become immune, or practically so, to its more virulent manifestations. That this generalization may be extended to bovine animals can be shown even more completely than might have been anticipated. If two calves, one a reactor and the other not, be inoculated with a lethal dose of tubercle bacilli, it is the reacting animal which survives and the non-reacting which perishes. This would seem to force the rather paradoxical conclusion that to resist tuberculosis a bovine animal must be tuberculous. If we are tempted to apply the principle to man the prospect is not alluring, and this, no doubt, is the underlying reason for the numerous researches into means for vaccination. The results obtained at Lille in this connexion are most interesting.

After thirteen years of consecutive cultures in presence of ox bile, an innocuous race of bacilli has been obtained. It is incapable of producing tuberculosis in man or animals. A small subcutaneous inoculation in calves causes no reaction, local or general, neither does it give any immunity. But a large dose (50 mg.) causes in calves within twenty-four hours an oedematous swelling about the size of a hen's egg. It reaches its maximum next day, and in the following days becomes a hard mass, firm and mobile under the skin. It slowly decreases in size, and disappears in about ten to eighteen months. The animal at first reacts positively to the tuberculin test, but becomes negative with the disappearance of the swelling. At Lille, in 1922, twelve calves were vaccinated thus, and six other animals were kept with them under identical conditions as controls. At intervals of one, three, six, twelve, fifteen, and eighteen months two of the vaccinated animals and one of the control animals were injected intravenously with a lethal dose of virulent bacilli. All the controls died of acute tuberculosis in less than two months, although none of the inoculated animals showed the slightest symptom. They were finally slaughtered and carefully examined, but no trace of the disease could be seen. Yet each animal had received 220 million virulent tubercle bacilli.

Similar and more recent experiments have been instituted with equally successful results, and Dr. Guérin is hoping to be able to conduct more in the future on a much larger scale. He proposes an annual inoculation at present—partly because the protection seems to last for under two years, and partly because the French agriculturist is accustomed to annual inoculations for anthrax and blackquarter, and so may be expected to have the greater confidence in the treatment. It is too early to predict the results of this experiment, but information will be eagerly awaited, not only by stock breeders on account of their own intrinsic interest, but by the medical profession also because of the possible future applications of the principle to the human disease.

THE PHYSIOLOGICAL PROBLEMS OF EVEREST.

MAJOR R. W. G. HINGSTON, medical officer to the Mount Everest expedition of 1924, lectured to the War Section of the Royal Society of Medicine on December 8th on the application of the efficiency tests of the Royal Air Force to the members of the expedition. Only the simpler tests could be carried out, because nothing in the way of complicated or bulky apparatus could be taken up the mountain. It was found that the power of holding the breath rapidly diminished with increasing height. One climber who had been able to hold his breath, after deep expiration, for 64 seconds at sea-level, was able to sustain only 14 seconds at 21,000 ft.; a similar reduction was found in all. At altitudes above 14,000 ft. shortness of breath was definitely felt. At 18,000 ft. any severe exertion made the breathing laboured and quick; at 21,000 ft. the very slightest exertion was accompanied by respiratory distress; at 27,000 to 28,000 ft. the breathing became so difficult that the rate of progress was incredibly slow, the climber had to rest for a minute or two every twenty or thirty yards, and a vertical ascent of 100 ft. was as much as could be accomplished in an hour's climb. Thus, although at the highest altitude reached the climbers were only 800 ft. from the summit, it would have taken almost a full day to cover that distance and get back to the highest camp. Oxygen was found to be of very little value in relieving the respiration; this was contrary to the experience of the previous expedition. With some of the party oxygen caused a drying of the throat which was very disagreeable in the rarefied air. It probably helped, however, to prevent frost-bite, and one climber, using oxygen at 27,000 ft., thought it helped to keep him warm. Instances of Cheyne-Stokes respiration were frequently noticed; it occurred only during sleep, and was very marked in illness. In some members of the party it occurred at an altitude of only 12,000 ft. Every climber suffered from inflammation of the respiratory passages, due to the inhalation of cold air. Some of the porters developed bronchitis and ulcerated throats, which made it necessary to send them down. With increasing altitudes the expiratory force tended to improve, but this probably had little to do with the function of respiration; it was merely an indication of the muscular power of the chest, which had been exercised as a consequence of the march across Tibet. There was a regular increase in the pulse rate in proportion to the altitude of the experiment, except at the very highest altitude of all, where the rate fell slightly. Signs of impaired circulation were seen at all the high camps. Cyanosis of the lips and lividity of the nails were frequently observed above 18,000 ft. Some members experienced giddiness, but there was no instance of actual fainting. All suffered from cold extremities. A number of cases of frost-bite occurred, both of the moist and the dry gangrenous type. There was no instance of haemorrhage from any of the mucous membranes. Headache was common at high altitudes—the result, it might be, of cerebral anaemia. It was increased by exercise, and especially by stooping to the ground, but was never severe. The natives of Tibet also experienced headache, even at quite comfortable elevations. Sleep was often difficult at high altitudes, but some members of the party slept fairly well at the highest camps. It was advisable to sleep with the head raised, as this helped to ease the breathing. The difference between the systolic and diastolic pressures diminished with the altitude—an indication of the strain on the circulation on reaching extreme heights. An increase in the red corpuscles was noted from 4,480,000 at 700 ft. to 8,320,000 at 18,000 ft. Tests revealed a diminution in the powers of endurance at each successive height, and a slowing of the pulse towards the end of the endurance test was increasingly marked with altitude. It appeared that the cardio-motor mechanism of a vigorous man at a height of 16,000 ft. closely resembled that of a physically inefficient person at sea-level. There was much loss of muscular power, and, above 15,000 ft., persistent wasting. Some members lost at least 2 st. in weight as a result of living for five weeks at an average height of 21,000 ft. Distinct dilatation of the heart was noticed, with a return to normal on reaching lower levels. Some of the climbers retained their appetite, though few enjoyed any solid food above 25,000 ft. Meat at high altitudes was particularly distasteful; sweet things were preferred. There was no nausea or vomiting. One member of the party at 19,000 ft. could not taste onions, and another could only just get the flavour of peppermint. Diarrhoea

in some cases was very persistent until the lower levels were reached. Though no impairment of judgement such as was described by airmen was observed, there certainly was mental lassitude and forgetfulness. One climber above 23,000 ft. had to record all his ideas, otherwise they would have been forgotten or distorted. At the highest altitudes all mental effort was a burden. No change in the knee-jerks was found. Three members of the party developed mild tremors of eyelids or fingers. Acclimatization seemed to increase, and those climbers who had been on two expeditions were unanimous in declaring that they suffered less on the second than on the first occasion. The physical deterioration during this expedition was increased owing to bad weather; a succession of gales and blizzards was encountered, so that the party was much debilitated before the final attempt was made. Yet they reached 28,000 ft. without oxygen, and, given good weather, Major Hingston was sure that the summit would be reached. There were no serious difficulties in what remained to be conquered of Everest; the problem was mainly physiological.

ANTHROPOMETRIC INVESTIGATIONS IN SCOTLAND.

THE volume comprising the second and third reports of the William Ramsay Henderson Trust consists of two monographs by Dr. J. F. Tocher.[1] In the first Dr. Tocher gives the results of an anthropometric survey of samples of the industrial population in north-east Scotland (Aberdeenshire, Banffshire, and Kincardineshire) as regards hair and eye colours, certain cranial measurements, and stature. In this survey 1,559 adult and adolescent civilians engaged in active work were measured, and the results are compared with those published in the first report of the Trust in 1905, relating to inmates of lunatic asylums in the same areas. Cranial measurements of 1,156 school children in Aberdeenshire and Glasgow are also included. The second monograph deals with a more extensive survey of the same characters in some 8,000 soldiers in Scottish camps carried out in 1915-16. The results obtained from those of Scottish birth are embodied in a series of maps which are of considerable interest. From a careful statistical analysis Dr. Tocher concludes that, though local differences in head height are of no significance, and those of head length are of doubtful import, the whole of the north of Scotland is definitely broad-headed, whilst the Lothians, Lanark, Ayr, Renfrew, and the Border Counties are distinctly narrow-headed; or, taking the cephalic index as criterion, the north-east is brachycephalic, the south and south-west dolichocephalic, Fife and Perth being buffer counties. This conclusion agrees with that previously reached from the study of asylum inmates. The northern counties are also found to be definitely superior in regard to cubical content of the head and in stature, whilst men from Lanark, Ayr, and Renfrew are below average in height. A comparison with the parallel measurements from insane adults shows small but significant differences in all the characters measured, but the author considers that the explanation is to be found, not in the domain of pathology, but in the fact that both the civil and military samples were in reality selected as regards stature and physique, and to some extent as regards age, and, moreover, the race admixture in the samples compared may not have been the same. The effect of growth on the cranial characters from the 18th year onwards is shown in a series of curves which closely resemble the growth curve for stature, reaching an adult level about the 23rd year, with little, if any, subsequent change. The volume consists, of course, principally of tables in which the actual measurements are given in full, and which have in themselves a permanent value. In accuracy, clearness, and style of production generally, the work maintains a high standard. In *The Origin and Distribution of Racial Types in Scotland*,[2] a monograph also published under the auspices of the Henderson Trust, Dr. John Brownlee summarizes the evidence already available relating to this question, such as the survey of Dr. Beddoe and the distribution of archaeological remains and of place-names, and then inquires how this evidence may be correlated with the findings of the anthropometric survey recorded in the three reports of the Trust. Most of this paper consists of the author's views or a discussion of those of his contemporaries on the controversial question of the sequence of invasions by differing racial types which has been responsible for the present population of these islands, and Scotland in particular. It cannot be said that any real relation between distributions of hair colour and cranial characters is proved by this survey. There is a curious mistake on pages 26 and 27, where the author erroneously concludes that a difference of 3.6 inches between the mean statures of two groups numbering 36 and 67 respectively is not significant from the standpoint of probability, whereas calculation shows this difference to be nine times its probable error. Apart from a good deal of conjecture the paper does not throw much further light on the explanations underlying Dr. Tocher's findings, but it must be conceded that the attempt to unravel the racial components by studying populations resulting after several thousand years of mixture and intermarriage is not one which justifies the expectation of startling discovery or affords a basis for much dogmatic assertion.

[1] *Measurements, Weights, and Other Facts relating to Man.* Henderson Trust Reports II and III. J. F. Tocher. Edinburgh and London: Oliver and Boyd. 1924. (8 × 11. pp. 172; illustrated. Printed for private circulation.)

MALARIA IN THE FEDERATED MALAY STATES.

THE annual report for 1923 by the secretary of the Malaria Advisory Board contains interesting information about the progress made in combating the scourge of malaria in the Federated Malay States. In our issue of October 11th (p. 677) we referred to the valuable work of Sir Malcolm Watson in Malaya, and the following details illustrate some features of the energetic campaign begun by him. A paper published in the *Review on Applied Entomology*, October, 1922, showed that mosquito larvae quickly die in rice fields which are temporarily drained, and the Board, therefore, has recommended the appointment of an expert to investigate this matter more fully with a view to action on a large scale. A model has now been prepared for exhibition, showing how to overcome the difficulty of providing inexpensive and effective openings, other than doors, into mosquito-proof rooms; models of beds illustrating the use of mosquito nets have been sent for exhibition to the principal manufacturing firms. Recommendations were approved by the Government and circulated to all mosquito destruction boards urging closer co-operation between health officers and these boards in different areas, so as to enable useful information to be more widely diffused. Every landowner is held responsible for the establishment of proper antimalarial measures upon his land, the railway department being responsible for railway reserves. The health resort of Port Dickson was particularly considered, and it was decided that all possible precautions should be taken to free the coastal area from malaria, and that antimalarial drainage should be started as soon as possible. Extensive propaganda work is being carried on. Posters dealing with malaria have been printed in various languages and circulated throughout the Malay States; lantern lectures have been delivered in English, Malay, and Chinese in almost all the schools throughout Selangor, pamphlets on malaria being freely distributed. During a

[2] *The Origin and Distribution of Racial Types in Scotland.* By John Brownlee, M.D., D.Sc., F.R.Anth.I. Henderson Trust. Edinburgh and London: Oliver and Boyd. 1924. (8½ × 11, pp. 29; 3 maps. Printed for private circulation.)

lantern lecture tour in Pahang thirty-three towns and villages were visited, and nearly 10,000 people—mainly Malayans—attended the lectures. Other districts have been similarly visited during the present year. In June an exhibit was arranged at the Malayan horticultural show in Kuala Lumpur, and was studied with great interest by 15,000 to 20,000 people. Included in the exhibit were photographs, charts, and models illustrating the method of reducing malaria by drainage, and instructions with reference to personal prophylactic measures. Educative information is circulated also through the local press from time to time.

RECENT ADVANCES IN PSYCHIATRY.

IN his presidential address to the Medico-Psychological Association of Great Britain and Ireland, delivered in the Queen's University of Belfast last July, Dr. M. J. Nolan dealt with certain aspects of our present-day knowledge of psychiatry and the application of this knowledge to the treatment of the insane.[1] Dr. Nolan avoided any detailed discussion of recent theories or modern therapeutic measures, preferring to deal with broad issues and enunciate general principles. The address was in the main philosophic and humanitarian. At the outset he contrasted the opposing views entertained as to the causation of mental disorder, and expressed the opinion that the acceptance of a purely physical or a purely psychical causation was an indication of a temperamental bias rather than the result of impartial observation and deduction—a bias towards a materialistic or a metaphysical interpretation of phenomena. This is reminiscent of William James's division of philosophers into the "tough-minded" and the "tender-minded"—those abiding by facts and those abiding by principles. Dr. Nolan cannot accept either hypothesis solely in any given case, preferring to believe in a combinaton of both as better explaining the origin of mental illness. Most cases, he thought, showed a clearly defined vicious circle of cause and effect, bodily and mental. Reviewing the problem first from the physical side, Dr. Nolan reminded his hearers of the store of specialized knowledge available dealing with abnormal conditions of the brain and general nervous system, of the blood, and of the endocrine and other organs in mental disease. This knowledge had thrown light on many dark places and afforded guidance in the treatment of the insane. Turning to the psychical side, he insisted on the necessity of a just estimate of the patient's point of view, and urged the asylum physician to put himself in the patient's place and pursue the line of morbid thought. True to this thesis, Dr. Nolan placed reliance on both physical and psychical methods of treatment, and would leave no avenue unexplored in the search for means of ameliorating the patient's state. When knowledge of the patient's standpoint has been obtained, "the line of suggestive treatment can be followed up; and this, coupled with such medical treatment as is suited to the special case from our investigation of the bodily condition, holds out the best prospect of amelioration, if not complete removal of the mental trouble." We are in hearty agreement with Dr. Nolan when he urges the intensive clinical study of the insane patient and advocates the closer co-operation of the general practitioner with those engaged in asylum practice. Dr. Nolan would discard entirely such a term as "alienist," which he considers has helped to bring about the estrangement between psychiatry and general medicine, and substitute that of "asylum physician." We cannot think, however, that a matter of nomenclature can be held responsible for the present state of affairs, nor that such a change would materially improve it. A closer and better understanding is more likely to accrue as a result of treating mental diseases in their earlier stages in general hospitals, as outlined in our article on September 27th, 1924 (p. 585), where we stated that "it is hardly possible to conceive of a more effective method of bridging the gulf which exists between psychiatry and general medicine than that which aims to treat mental cases in a hospital dealing also with surgical and medical cases."

[1] *Journal of Mental Science*, October, 1924.

AN OSLER MEMORIAL VOLUME.

THE Osler memorial volume, which the International Association of Medical Museums has had for some time in preparation, will, we learn from a notice issued a few months ago, contain some 300 pages and will be freely illustrated. In meeting the cost of publication the association is being helped by members of the late Sir William Osler's family and by numerous other friends. The subscription list is still open, and Dr. Lydia M. Henry, who had an opportunity of inspecting the list at McGill University, Montreal, last summer, informs us that it contains relatively few names from Great Britain. This is, no doubt, due to the fact that the project is not well known on this side of the Atlantic. Dr. Maude E. Abbott of McGill University is the managing editor, and will receive subscriptions. Dr. Henry further informs us that the volume is now nearly ready, and that subscriptions ought therefore to be sent at once. The sale and subscription price is 10 dollars. A number of Sir William Osler's friends and pupils have made contributions; among them we notice the names of Sir Clifford Allbutt, Professor W. Welch, Sir Arthur Keith, Colonel Fielding H. Garrison, the late Sir German Sims Woodhead, Dr. Thomas McCrae, Dr. J. G. Adami, Dr. Howard Kelly, Professor Llewellys F. Barker, Sir Humphry Rolleston, Sir William Hale-White, Sir Seymour Sharkey, and Dr. William Collier and Dr. A. G. Gibson of Oxford. The matter will be arranged under various headings: first there will be general articles and contributions; next, an account of Osler's early life; then recollections of his work in Montreal, in Philadelphia, and finally in Oxford. A classified bibliography has been prepared by Dr. Maude E. Abbott, Dr. Henry Cattell (Washington), Dr. E. B. Krumbhaar (Philadelphia), and Dr. Archibald Malloch, with the assistance of Miss M. W. Blogg, who published a chronological bibliography of Sir William Osler's works in the *Johns Hopkins Hospital Bulletin* (1921).

FOOD PROBLEMS IN RELATION TO INDIA.

THE influence of diet upon physical efficiency, as illustrated among the different races of India, was the subject of a lecture given by Lieut.-Colonel R. McCarrison, C.I.E., I.M.S., before the Indian Section of the Royal Society of Arts on December 5th. He said that anyone who had travelled far in India could not have failed to observe the remarkable differences in the physical efficiency of its various races. Although a number of factors, climatic and other, played their part in determining those differences, yet there could be no doubt that nutrition was the principal. Such foods as rice, maize, barley, wheat, millet, legumes, vegetables, and fruits formed the food of millions of Indians; animal foods, milk, and milk products were, as a rule, taken only by the better-to-do classes and by certain races of Northern India. The commonest and poorest of all the dietaries consisted of rice, legumes, vegetables, and condiments; it was a diet poor in protein, in certain mineral elements, and in certain vitamins, and the people whose diet it was had the poorest health and physique and the lowest powers of resistance to bacterial infection among any of the races of India. The people in the United

Provinces had a similar diet, but wheat was introduced, either in addition to or in substitution for rice. This diet, although deficient in certain respects, was an improvement on the rice-legume diet because wheat furnished a better protein and more of it, and the people in consequence had a higher degree of physical efficiency and general well-being. Among races which, like the Rajputs, the Sikhs, and the Pathans, supplemented the cereal and vegetable diet with animal foods, especially milk and milk products, the highest degree of physical efficiency among Oriental races, or, indeed, among any of the races of mankind, was found. The diet of the Sikhs was lacto-vegetarian, and contained all the elements and complexes necessary for the maintenance of perfect nutrition. Throughout life the Sikhs used milk and milk products freely; they added meat occasionally, but it was a luxury rather than a common food. It would be difficult to conceive a diet which more adequately fulfilled the functions of food than that of the Sikhs, and those who were familiar with this race would acknowledge that for courage, manly qualities, and general physical efficiency they were unsurpassed. The diet of the Pathans was much the same as that of the Sikhs, but they ate more meat; they, too, were among the finest races of mankind. Colonel McCarrison referred in particular to the people of the Hill State of Hunza, in the extreme north of India; the diet of these people corresponded to that of the Sikhs, but included an abundance of fruit, especially apricots and mulberries, both raw and sun-dried. These people had exceptional powers of physical endurance, gastro-intestinal disease was extraordinarily uncommon among them, and they retained vigour to a very old age. Colonel McCarrison went on to speak of the connexion between certain rice foods and beri-beri, and described the experiments, already fully related in these pages, on pigeons fed on different rices. He led up to the conclusion that the fault in common dietaries was not solely vitamin deficiency, but an excess of starch or sugar, or both, or a deficiency of essential mineral elements. The results of such a combination of food faults as were found in the dietaries of Europeans as well as most Indians were exemplified in the prevalence of gastro-intestinal disease. At the close of his lecture Colonel McCarrison touched upon the interesting problem, discussed by him in our pages of March 29th, 1924 (p. 567), as to whether the food values of various rices in common use in India and the nutrition and vitamin value of cereals varied with the conditions of their growth. It had not been possible to put this to the test in the case of rice, but some experiments were made in the case of millet, another staple grain, and it was found that millet grown on soil with natural cattle manure was more nutritious and contained more vitamins than that grown on soil manured artificially; and, as was to be expected, millet grown on soil exhausted of manure was even worse than that grown on soil artificially manured. In a brief discussion following the paper Sir William Willcox spoke of his experiences in Mesopotamia during the war, where the British troops were fed on bread and biscuits (made of the usual sort of highly milled flour), jam, and tinned meat, with no fresh milk or vegetables, and on this diet developed beri-beri, although the small amount of fresh meat obtainable protected them from scurvy. The Indian troops, on the other hand, had a diet of wholemeal flour, lentils, and no meat, and their diet protected them against beri-beri, though not against scurvy. He emphasized the importance of educating public opinion in the matter of diet, especially as to the necessity of eating sufficient fresh and uncooked foods and bread made of whole flour. Dr. C. Muthu deplored, as did other speakers, that the Indian Government, for financial reasons, had withdrawn Colonel McCarrison from his work in India, and hoped that a way would be opened to send him back to complete his labours.

MILK AND ARTERIO-SCLEROSIS.

THE suggestion that there may be some relation between the ingestion of milk and cheese, with their high calcium content, and the production of arterio-sclerosis was made in a letter in our columns of December 6th (p. 1082). In this connexion it may be interesting to note that W. J. Orr, L. E. Holt, jun., and others, joint authors of a paper in the *American Journal of Diseases of Children*, November, 1924 (p. 574), produce evidence in favour of the view that excessive amounts of calcium in the diet tend to increase the total absorption and retention of it, impairing phosphorus retention. They believe also that an increase of phosphorus in the diet accelerates the excretion of calcium. Considerable controversy has arisen on this matter, especially in relation to rickets. Experimental work has generally confirmed the view that active rickets is associated with defective absorption of calcium and phosphorus, and that as improvement occurs large quantities of these elements are retained in the body. Blühdorn, however, concluded, from the experimental study of the metabolism of two children, that calcium and phosphorus absorption ran parallel, and that an excess of either element in the diet caused an increase in the quantity of both retained in the body. Orr and his colleagues thereupon made a careful study of two children, one with incipient rickets and the other without, and produce evidence that increasing the intake of calcium causes calcium retention; the serum content of calcium rises if it was previously low, while at the same time more calcium is excreted in the urine. Phosphorus retention is simultaneously diminished, a considerable amount of phosphorus being diverted from the urine to the stools; the content of phosphorus in the serum is decreased. The effect of increasing phosphorus in the diet they found to be an increasing concentration of phosphorus in the serum; a very marked decrease in the amount of calcium retained; a great increase in the calcium in the stools; a marked diminution of the calcium in the urine; and, in one case, a definite fall in the calcium content of the serum. Orr suggests that these phenomena arise from the precipitation of insoluble phosphates of calcium in the intestine, and, if this is true, any factor, such as increased acidity of the intestinal contents, which favours solution of calcium phosphate will promote the absorption of both calcium and phosphorus from the intestine. Orr has obtained evidence that calcium absorption is decreased by diets increasing the alkalinity of the stools. Further experimental work is necessary before definite deductions can be made, but it certainly appears from the report of Orr and his colleagues that the prevention of calcium absorption by the body may not be a very difficult matter.

BIOCHEMICAL NOMENCLATURE.

THE International Union of Chemistry, Pure and Applied, has a Committee on Biochemical Nomenclature, which presented a preliminary report to the conference at Copenhagen in June last. The report was adopted. It reiterates the recommendation that the name given to a substance the constitution of which is very complex or imperfectly known should have a termination corresponding to the principal chemical function, and that the termination "ine" should only be used for immediate principles containing basic nitrogen. It advises that the glucides should be formed into two groups, that the term "glucose" should be applied to a reducing non-hydrolysable glucide, and that another name should be found for a glucose giving on complete hydrolysis one or more glucoses. The names of all glucides should terminate in "oside"—that is to say, salicine should become salicoside and amygdaline amygdaloside. The names of all lipides should terminate with "ide"; thus stearine is to become stearide and palmitine

palmitide. A further report on the nomenclature of proteins and diastases or enzymes is to be made to the next (the sixth) conference of the union.

THE SOCIETY OF APOTHECARIES.

THE winter livery dinner of the Society of Apothecaries of London was held at Apothecaries' Hall, Blackfriars, on the evening of Tuesday, December 9th, with the Master, Dr. Algernon D. Brenchley, in the chair. The members of the Society and their guests were received by the Master and Wardens in the finely proportioned Court Room, and dinner was served in the charming old Livery Hall. The original Apothecaries' Hall, it will be remembered, was destroyed in the Great Fire of London, and the present group of buildings forms a good example of seventeenth century architecture. The medical banquets held there are looked forward to with more than common pleasure, for the hospitality of the Society is in keeping with its old-world habitation. On the present occasion many distinguished members of the medical profession attended, notwithstanding the dense fog. The toast of the Navy, Army, and Air Forces was proposed by Mr. Samuel Osborn, F.R.C.S. (recently appointed to represent the Society on the General Medical Council), who acted as Deputy Senior Warden in the regretted absence, through illness, of Dr. Vincent Dickinson. It was responded to by Surgeon Vice-Admiral Joseph Chambers, C.M.G., Medical Director-General R.N., and by Lieut.-General Sir William Leishman, K.C.B., F.R.S., Director-General Army Medical Services, who each spoke of the continued efforts of their departments to maintain efficiency with sadly depleted personnel, and asked for the co-operation of the civil profession in maintaining their great traditions. The toast of the Royal Colleges and the Universities was proposed in historical vein by the Master, and was replied to by Sir Humphry Rolleston, Bt., K.C.B., President of the Royal College of Physicians of London, and Dr. E. Graham Little, M.P., who represents the University of London in the new House of Commons. The health of the guests was proposed by the Junior Warden, Dr. Alfred Hepburn, and was responded to by the Right Hon. Sir Henry Craik, K.C.B., M.P., and by Mr. Walter G. Bell, F.S.A., whose interesting book on *The Great Plague in London in 1665* we discussed in our last issue. The after-dinner speeches were interspersed with delightful songs and violin solos.

THE Schorstein Memorial Lecture will be delivered by Sir Arthur Keith, M.D., LL.D., F.R.S., on Wednesday, December 17th, at 4.15 p.m., in the anatomical theatre of the London Hospital Medical College. His subject is the clinical importance of the bulbus cordis. The lecture will be illustrated by lantern slides. Members of the profession are cordially invited to attend.

THE first meeting of the Royal Commission appointed to inquire into the conditions prevailing in the wholesale and retail trades in articles of food of general consumption so far as they affect prices, was held on December 10th, when the Right Hon. Sir Auckland Geddes, G.C.M.G., K.C.B., M.D., was in the chair. Preliminary arrangements were made for the conduct of the inquiry.

SIR JAMES MACKENZIE, M.D., F.R.C.P., F.R.S., Honorary Physician to the King in Scotland, has been awarded by the Council of the Faculty of Medicine of the University of Toronto the Charles Mickle Fellowship of that university. The fellowship is awarded annually to that member of the medical profession who is considered by the council to have done most during the preceding ten years to advance sound knowledge of a practical kind in medical art or science. The annual value of the fellowship is about 1,500 dollars.

A SEVERE epidemic disease of the central nervous system has occurred recently in Japan, and 6,551 cases were reported up to the end of September. The case mortality rate is 54.85 per cent.; the symptoms resemble those of encephalitis lethargica, without the eye complications, and the persons affected have been mostly over 50 years of age. Although the disease was widespread, rarely more than two cases occurred in the same family. No pathogenic micro-organism has been isolated, and the cerebro-spinal fluid was clear in all cases. The summer has been unusually dry, and the epidemic began to decline when the autumn rains started.

Medical Notes in Parliament.

[FROM OUR PARLIAMENTARY CORRESPONDENT.]

The King's Speech.

THE Speech from the Throne was read by the King in person on Tuesday, and debate on the policy of the Government, outlined in the speech, began in both Houses that day. It continued on Wednesday, and will be resumed on the first three days of next week. The King's speech, besides setting out the legislative programme for 1925, also indicates further lines of possible action which are now being investigated. A promise is made to reduce public expenditure to the lowest limit compatible with the security and efficiency of the State, but promises are given to continue measures of alleviating the distress due to unemployment, to promote the development of the public system of education, and to encourage the rapid production of houses by new methods. Passages more definitely bearing on public health declared that "the housing problem is not merely one of overcrowding, but also of the existence of large numbers of houses which fall below modern standards of propriety and sanitation. Something is already being done under existing legislation to clear the worst areas and to remedy sanitary and structural defects in those which remain. As new building increases it will be possible largely to develop this process, and my Government will take every opportunity of pressing it forward with vigour. My Ministers are engaged upon inquiries into the possibility of combining with existing systems of insurance provisions for improved old age pensions and pensions for widowed mothers. Steps will be taken to carry out the principal recommendations of the committee on the use of preservatives and colouring matter in food, and for the ascertainment of costs and profits in the distribution of milk."

Bills are promised during this session dealing with medical examination and stokehold employment of young seamen, and to amend and consolidate the Factory and Workshop Acts and the law with regard to the use of road vehicles.

In the opening stage of the debate on the Address late on Tuesday night, Dr. F. E. Fremantle obtained the ear of the House for a succinct statement of the objects for which medical members of Parliament thought the Government should work in the present Parliament. Dr. Fremantle drew the Government's attention to the fact that the death rate for tuberculosis in this country had been reduced to one half of what it was thirty or forty years ago. If the other half of the death rate could be removed the annual saving of national income would be some £90,000,000, and about £5,000,000 Exchequer expenditure. A Milk and Dairies Bill to eliminate tuberculous milk had been passed at the beginning of the war, but had never been enforced. It ought to be re-enacted with modifications. The medical profession, with its veterinary allies, said that tuberculosis could be eliminated in ten or twenty years. In welcoming the references to housing in the Speech from the Throne as an important step towards improved national health, he referred to the activity of the building societies, which had built 295,000 houses in five years, mostly without subsidy. He pointed out, however, that if the building programme proposed by the late Government were continued for ten or twenty years great blocks of houses would appear all over the country. It was important to consider their distribution. Town-planning schemes had lagged behind, and he hoped that the Government would insist on town planning for all areas and also introduce new legislation applying town planning to areas already built up, but which were being pulled down or should be pulled down. All over London the houses were coming to the end of their life, and they required a scheme for their replacement on proper town-planning lines. Dr. Fremantle enumerated other health services which the Government should deal with. The bill codifying factory laws would not suffice. The factories should be linked up with the present health system of the country. It was inevitable that they should have separate health services under the Ministry of Health, the Home Office (for factories), and the Ministry of Education, but those services should have a common policy. He hoped that the medical profession would make a large contribution to the solution of the problems which confronted the Royal Commission on Health Insurance and also to those raised by the hospital system. In conclusion he referred to the Indian Medical Service as affected by the Lee report, and to the perilous position of the Army Medical Service.

The medical members of Parliament will dine together at the House of Commons on Wednesday, December 17th, to enable the medical members who have sat in previous Parliaments to welcome their new colleagues.

ST. GEORGE'S HOSPITAL.

NEW OUT-PATIENT DEPARTMENT.

ON the afternoon of Friday, December 5th, Sir Humphry Rolleston, Bt., K.C.B., M.D., President of the Royal College of Physicians, and Emeritus Physician to the Hospital, formally opened the out-patients' department extension at St. George's Hospital, and unveiled the bust of John Hunter which has been erected over the new gateway leading into the Medical School.

SIR HUMPHRY ROLLESTON'S ADDRESS.

In declaring the new out-patient department open Sir Humphry Rolleston said:

St. George's Hospital—an offshoot from the Westminster Infirmary or Hospital (founded 1721), just as St. Mary's Hospital was from St. George's—was the outcome of disagreement in the parent institution, thus illustrating the dictum that "something good will be the final goal of ill." Our hospital, which had the advantage of a healthier as well as a wealthier neighbourhood, first opened its doors on January 1st, 1733-34, and during its life of 190 years has, of course, seen many changes and developments. At first an existing building, Lanesborough House, was utilized, and a staff of six physicians and three surgeons, shortly modified to four of each, "declared their willingness to serve without fee or reward," an obligation that has been loyally observed ever since. Starting with but 30 beds, the number was soon doubled, increased when the hospital was enlarged, about 1767, to 200 beds, in 1834 to 325, and at the present time is 342. The voluntary hospitals of London have had a struggle of increasing severity, especially during this century and since the war, and unfortunately St. George's, in spite of its prosperous surroundings, is no exception to this rule, for at present the deficit between the assured income and the normal expenditure is £20,000 a year; it is, however, obviously an imperative duty to keep the hospital efficient, and hence a new out-patient department has become absolutely essential, even in the face of financial straits and the very considerable additional expense entailed by this necessary extension.

When a definite out-patient, as apart from a casualty, department was first inaugurated appears uncertain; during the hospital's first year of existence (1734) the number of out-patients was 258, in 1821 there were only 2,018, in 1851 they rose to 9,417, in 1886 to 23,180, and in 1923 the total of new out-patients was 34,028. Before 1893, when a third assistant surgeon and a third assistant physician, in the persons of the late Mr. A. Marmaduke Sheild and myself, were appointed, out-patients were seen on four days only in the week, and the change to six days a week has no doubt helped to swell the number of attendances, but the expansion of the special departments has been a noticeable and satisfactory feature in the activities of the out-patient service.

The out-patient department which is formally opened to-day is a great advance on that erected in 1891, just as that one was on its predecessor, which existed when I first came to St. George's. The new and attractive arrangements now available, thanks to the skill of the hospital architect, Mr. Percy Adams, and the advice of the active medical and surgical staff, await your inspection and approval and need not be described in detail, but it would not be right to pass in complete silence the excellent operating theatre unit with its generous supply of waiting and recovery rooms.

The Functions of an Out-patient Department.

The out-patient department (Sir Humphry Rolleston continued) has a number of functions; besides its primary one of providing relief to the sick poor, it is important that it should be a centre for consultation with general and panel practitioners, who can thus obtain an opinion by letter; I understand that more than half the out-patients come with this object. Further, the out-patient department has a great scope in teaching; the patients there are much more like those seen in ordinary practice than those admitted to the wards; but being often in an early stage of disease do not present the physical signs which are produced later by the morbid process responsible in the first instance for the symptoms. The study of these symptoms—that is, the manifestations of commencing disease—depends so largely on the patient's mentality and ability to describe his sensations that it is much more difficult than examination for the detection of physical signs, in which we rely on our own powers, and with which, it must be admitted, we are more inclined to be satisfied. Patients in the wards are to a great extent admitted there in virtue of either their physical signs or grave illness, and being usually in a more advanced stage of disease are therefore less amenable to cure than to temporary relief. The out-patient, on the other hand, is in a better position to benefit from the practice of the highest form of medicine—the ideal of the prevention of disease.

Unveiling of John Hunter's Bust.

At the unveiling ceremony beside the new gateway of St. George's Hospital Medical School Sir Humphry Rolleston said:

John Hunter, that wonderful genius, was born so much in advance of his time that even now, with the 200th anniversary of his birth due for celebration in 1928, he has hardly been overtaken. A pioneer in anatomy and physiology, an experimental philosopher, and the founder of scientific surgery, his name is immortal and needs no material memorial. It is our proud heritage to belong to the hospital where he did so much, and incidentally instituted clinical teaching; and this statue should serve to stimulate us, as we pass in and out of the Medical School, to follow, however distantly and imperfectly, his noble example of whole-hearted devotion to the sciences he so greatly adorned.

NEW YORK HOSPITALS.

A SURVEY of the hospitals of New York City has been undertaken by the Public Health Committee of the New York Academy of Medicine. Its report, prepared by Dr. E. H. Lewinski-Corwin, is published in a handsome volume[1] which deserves to be studied by members of hospital staffs and boards in this country as well as in the States.

New York hospitals had their beginning in one room of six beds set apart for an "infirmary" in the "Public Workhouse and House of Correction" built in 1736. It was the forerunner of the Bellevue Hospital. "A survey of the trend of hospital development since then," we are told, "reveals the same lack of conscious directing force which characterizes that of nearly every branch of social evolution. Scientific enthusiasm and individual initiative, generous benevolence and the rapidly extending city boundaries, combined to beget in rapid succession hospitals of one kind or another." The greatest growth of hospitals dates from 1850. There are now 32,000 beds, representing a capital value of sixteen million pounds sterling, not counting equipment. Of the 182 hospitals which contain these beds, 18 are municipally owned and contain 39 per cent. of the beds, 114 were founded by private philanthropy and hold 57 per cent. of the total, whilst 5 per cent. are in proprietary institutions. About 60 per cent. of the beds are in general hospitals. Special hospitals came into existence as a protest against the neglect or refusal of general hospitals to treat certain classes of cases. Now conditions have changed, and it is held that (except for orthopaedic and neurological cases which are very chronic) the special hospital has reached its zenith. The preferential value of small or large hospitals is debated; it is found that the small hospitals are more difficult to staff, and would be more expensive if they took the same class of work as the large.

In the past it has been usual for the hospital staff to serve in rotation for only part of a year. A tendency for all-the-year service has rendered it imperative for the

[1] *The Hospital Situation in Greater New York.* Report of a Survey in New York City by the Public Health Committee of the New York Academy of Medicine. Prepared by E. H. Lewinski-Corwin, Ph.D. New York and London: G. P. Putnam's Sons. 1924. (Med. 8vo, pp. xvi+356; illustrated.)

hospitals to provide their attending staffs with private and semi-private rooms, sufficient for the accommodation of all their private patients. In non-municipal general hospitals 70 per cent. of beds are in public wards, and the rest in private or semi-private rooms. The proportions vary greatly. In some hospitals physicians are allowed to charge patients in the public wards; this is not considered a desirable practice. Of the medical practitioners of the city 36.8 per cent. are on the staffs of hospitals; on an average there is about one staff member to every ten beds. Ambulances are maintained by both municipal and independent hospitals, the latter providing the larger proportion. The ambulance problem is under discussion, and the view is held that it should be a municipal service.

The data provided by the medical statistics are fragmentary, and do not afford more than an approximation to an index of disease incidence in the community. Mortality statistics, it is held, are not an adequate index of hospital needs, but the decline in the death rate is considered to justify the assumption that there has been a reduction in the prevalence of disease, and therefore of the need for hospital provision. The present accommodation provides a bed for every fourth sick person. There is still a prejudice against going to hospital, but domestic service difficulties are reducing it. Formerly not more than one-tenth of the sick persons went to hospital. Unlike London, the vast majority of fever cases do not go to hospital. There is an increasing tendency for hospitals to be used for obstetric cases, consequently the midwife as a factor in the situation is gradually declining in importance. In 1909 midwives attended 40 per cent., in 1920 only 26 per cent., of all mothers.

The utilization of hospital beds varies widely—from as high as 100 per cent. to as low as 32 per cent. Conditions of prosperity affect the demand; when the people are well off they go less to hospital. Compensation (remuneration) for interns (house medical staff) averages 10 to 25 dollars a month with keep and uniform. Surgical cases constitute 60 per cent. of all general hospital patients, and in these are included 24 per cent. gynaecological. Charges vary from 3 to 15 dollars a day; the higher priced rooms constitute 13 per cent. Most charge extra for laboratory work, for *x* rays, for extra drugs, for the operation theatre, and for special nursing. Fever hospitals show a very light demand; only 31 per cent. of the beds are used, but with seasonal variations in these diseases this is inevitable. Hospitals for children and infants show a light demand, and this is held to be undoubtedly directly related to decreasing mortality. Maternity beds are much more used in private hospitals than in municipal hospitals.

The only overcrowded hospitals are those dealing with mental cases; the State mental hospitals show an average utilization of over 130 per cent. There is need for a psychopathic hospital in which mental cases may be treated without the stigma of commitment.

The greatest need of change in hospital policy lies in regard to the "middle classes." Their standards are such that they dread the atmosphere of the public hospital ward, and the idea of charity and free medical services is repugnant to them. This raises the question whether charges should not be regulated by the income of the patients, even if the private pavilion should thereby cease to be a money-maker for the hospital.

The ratio of lay staff works out at one and a half to each patient. It is much higher for the Presbyterian Hospital and lower for the Misericordia hospitals, where work usually done by maids is performed by the sisters.

Costs are mounting heavily, but income also has increased. The general prosperity and the insistence that those able to pay should pay has greatly decreased the amount of free treatment. Voluntary contributions have increased, but they are now relatively less than patients' payments. The "United Hospital Fund" is increasing in popularity, and more benefactors give to hospitals through this fund than formerly instead of subscribing directly to a given hospital. Endowments form a problem. Most carry the right to nominate to the bed endowed. This would mean a grave loss if it were fully acted upon, but it is not. There is some thought of qualifying the terms of the deed. Increase of income arises also from workmen's compensation cases. The law provides for compensation to be paid by the employer or insurance company to physicians and the hospitals for the care and treatment given to injured workmen or those suffering from disease of occupational origin. In some attending physicians make their own arrangements with the employers for the care of their cases in the hospitals, in most there is an established scale. The State Board of Charities has recently put it on record that cases paid for under the workmen's compensation law are not suitable for treatment in dispensaries unless urgent. Free days (the count is in days and not by beds or patients) average about 30 per cent.

The independent hospitals are managed by boards, usually lay, sometimes with medical members, in others with a liaison medical representation. All the staffs, lay and professional, record their attendances, and default leads to disciplinary action. The mode of admission of patients varies; in some hospitals 90 per cent. of patients are referred by the members of the visiting staff, while in one only 22 per cent. were so referred. It is suggested that there is need for a systematic inquiry into this matter, and into the lapse of time between recommendation and admission.

The whole problem of financial control of expenditure is undergoing evolution, and there is a strong move in the direction of accountancy by "costing," so ably advocated by Mr. Stone, the accountant of St. Thomas's Hospital, London. Most detailed and valuable information is tabled on the equipments of the various hospitals. The common form of annual report is condemned; it is too much and too dry for the public, too little for professional uses; lines of reform are suggested.

Medical organization is dealt with in a special chapter. It is claimed that "the medical staff is the most important functional group of the hospital—its alpha and omega. The selection of the medical staff therefore constitutes the greatest responsibility of hospital management." There are three types of staffing:

1. All the attending doctors have alternating services, all are of equal rank and serve in rotation for a certain number of months in the year. It is the most common type and the least satisfactory. It limits research and responsibility. But it has the advantage of increasing the number working at hospital.
2. There is a head director who is all-powerful: he selects his assistants and assigns their work. This plan has the advantage of centralization and definiteness of responsibility. The disadvantages are in proportion to the personal limitations of the director.
3. The third type is that common in the United Kingdom—namely, regular staffs, continuous service, each chief of a department directing the work of his staff. It ensures continuity of treatment, good control of the house staff, good teaching and research work, and it links up the out-patient work with the ward work. But there is no "closed" staff, as every hospital has a so-called "courtesy" staff of men who have the privilege of sending in private patients and attending them at the hospitals; but with the exception of the proprietary hospitals there are no absolutely "open" hospitals.

The relation of the out-patient department to the hospital has not been worked out satisfactorily, but the tendency is to link them together, so that unified organizations where hospital appointments carry with them responsibility for out-patient work is increasing. Most hospitals have an age limit for retirement—from 60 to 69 years. In most hospitals assistant physicians are required to make complete daily rounds of the patients, and in thirty-seven this is also required of the chief of service. Efficiency tests of hospital work are increasing, mainly owing to the work of the American College of Surgeons and the American Medical Association. Staff meetings are becoming more frequent, and medical records are being improved. In many of the better hospitals the appointment to the internships are on a basis of a competitive examination, in others on credentials. The nursing situation is dealt with in detail; and a very long chapter is given to the analysis of hospital medical records.

There is a final chapter on a "community policy" which is most illuminating, and would serve as a text for a discussion on the hospital problem in this country equally well. The book is a fine piece of work, clearly written, restrained and balanced in statement, and amply documented by facts and figures.

Nova et Vetera.

KINGS AS PHYSICIANS.

AFTER reciting instances culled from every part of the world, Sir James Frazer (*The Magic Art: Golden Bough*, vol. i, p. 371) concludes that

"On the whole, then, we seem to be justified in inferring that in many parts of the world the king is the lineal successor of the old magician or medicine-man. When once a special class of sorcerers has been segregated from the community and entrusted by it with the discharge of duties on which the public safety and welfare are believed to depend, these men gradually rise to wealth and power, till their leaders blossom into sacred kings. But the great social revolution which thus begins with democracy and ends in despotism is attended by an intellectual revolution which affects both the conception and the function of royalty. For as time goes on, the fallacy of magic becomes more and more apparent to the acuter minds and is slowly displaced by religion; in other words, the magician gives way to the priest, who, renouncing the attempt to control directly the processes of nature for the good of man, seeks to attain the same end indirectly by appealing to the gods to do for him what he no longer fancies he can do for himself. Hence the king, starting as a magician, tends gradually to exchange the practice of magic for the priestly functions of prayer and sacrifice."

The last of the instances cited by Sir James Frazer was that of the Tonga chiefs, who both healed and caused scrofula by their touch. The last but one was the Royal Touch of the monarchs of France and England.

This subject has a perennial attraction. Macaulay devoted to it some of his most trenchant pages—pages wherein the notes of the hammer, perhaps hitting the right nail on the head, re-echo merrily. Dr. Raymond Crawfurd dedicated a scholarly monograph to it, and now another scholar—Professor Marc Bloch of the University of Strasbourg—has composed a volume of more than 500 pages on the same theme.[1]

Professor Bloch has combined a Teutonic assiduity of research with something of the Gallic charm of style; it is hard to believe that even a German *Privatdozent* will discover "sources" untapped by Professor Bloch, who has, moreover, written pages which are not unworthy of a master of urbane and learned scepticism whom the world now mourns—his great fellow citizen Anatole France. Professor Bloch has not, indeed, the practical acquaintance with the psychology of ailing humanity which informs such a work as that of Dr. Crawfurd, but he has cast his net somewhat wider. The reader of the two books will certainly know all that anyone needs to know of a curious chapter in the history of mankind.

Just as the embryology of an animal recapitulates its phylogeny, the history of a custom or belief extrinsically of no more than local importance is intrinsically a mosaic of conscious and unconscious motives forming a typical pattern. The ground colour of the pattern is provided by unconscious motivation—beliefs handed down from a remote past and accepted not only without question, but without anyone being able to conceive that they would brook question. This is the ground colour, but tesserae of the mosaic, not indeed very numerous, but disposed in a reasoned order, strike the eye by the brightness—or sombreness—of their hue, and stand up against the grey backing. These are conscious ambition, craft, and fraud. The history of the Royal Touch illustrates all these points.

In its origin this healing power was part of the divinity either inherent in or grafted upon the kingship. Professor Bloch rejects, it would seem with reason, some of Sir James Frazer's analogical arguments; in its origin the healing power of the Capetian dynasty was not, so to speak, wholly official; there was perhaps a eugenic element, but the Strasbourg professor fully recognizes the importance of the psychological correlation between kingship and magic powers. When, from the beginning of the eleventh century in France and from about a century later in England, the miracle becomes of importance, it is possible to trace the introduction of other elements. Perhaps Philip I, and still more probably our own Henry I, consciously exploited a belief that had an evident political importance; from thence onwards we can observe the working of both conscious and unconscious motivation. The prestige of the royal miracle worker is enhanced by the attribution of mysterious properties to the oil with which he has been anointed; there is partly a deliberate and partly an unconscious confusion between his inherent powers and those conferred by his anointing—a confusion which soon led to difficulties when the royal power and the papacy were in conflict. In France again, partly deliberately and partly unconsciously, the royal magic is associated with the legend of an individual miracle worker. Professor Bloch's statement of his views may be thus translated:

> In France there were from about the eleventh century kings who cured *les écrouelles* (scrofula); there was also in the same country a century or two later a saint who was believed to possess a similar power; the disease was both the "royal disease" and "the disease of Saint Marcoul." How was it to be supposed that these two wonderful facts did not stand in some relation to each other? Imaginations looked for a connexion, and because it was looked for it was found.

Finally, in the complex pattern we find indications of plain, simple lying, as in the invention of the Clovis myth by Forcatel in 1579, the enormous success of which proved, says Professor Bloch, that it supplied a sort of logical need of the popular imagination.[2]

The end of the story makes it clear that of the various ingredients the primitive belief in a quasi-hereditary virtue of the dynasty was the most important. In England the miracle expired with the king *jure divino*; we have far too high an opinion of the astuteness of William of Orange to believe that he would have refused to touch for the evil if he had supposed for a moment that the ceremony would have had any of the value in a political sense that our Angevin kings probably attributed to it. The substitution of a king by Act of Parliament for a king by divine right deprived the miracle of its psychological basis. In France it survived another two generations, until the collective belief in the kingly status had died out.

We have left to the last the most interesting of all the questions—namely, whether the king as a physician was a successful physician; upon this point we find Professor Bloch hardly more illuminating than our Whig historian. Both of them leave many joints in their harness pervious to the shafts of a Gilbert Chesterton. Professor Bloch tells us, with obvious truth, that to suppose the royal touch could cure a tuberculous adenitis is to defend a physiological heresy; with equal truth he remarks that the medical men of those days had neither a rigorous terminology nor exact methods of diagnosis, and he reaches the conclusion that "it is difficult to see in the belief in the royal miracle anything more than the result of a collective error, an error, as a matter of fact, much less injurious than most of those with which the history of mankind is full." For our part we find it difficult not to see in this reasoning an instance of the familiar round game, *chercher raison où il n'y en a pas*.

The real problem is surely whether it is not true that "miracles" *do* happen when one expects them to happen—whether, in fact, the royal physicians did not really cure *some* patients so long as the patients believed they could cure them. None of Professor Bloch's or of Macaulay's arguments are addressed to this point. They are vitiated by a neocentric error, that of substituting a technical connotation of recent date for a vague connotation of ancient date. It is idle to show that scrofula or *écrouelles* as defined by a modern pathologist cannot be cured by a witch-doctor. The experience of "Christian scientists" and other "witch-doctors" in our own time makes it extremely probable that the royal medicine-man, whose prestige was surely as great as even Mrs. Eddy's, did "cure" many patients.

The real case against miracle workers is that nowadays

[1] *Les Rois Thaumaturges. Étude sur le caractère surnaturel attribué à la puissance royale particulièrement en France et en Angleterre.* Pp. 542. Strasbourg and Paris. 1924. (London Agents, Oxford University Press.) Price 12s. 6d. net.

[2] The healing by the touch of Clovis of a scrofulous squire; the names used by Forcatel show, says Professor Bloch, that he was not even well acquainted with the vocabulary of the Merovingian age.

we have for so many diseases better remedies than resort to the miraculous. The reason why all intelligent medical men protest against the miraculous treatment of, say, cancer, whether by the laying on of hands or by the use of violets or vitamin-free diets, is that these procedures delay or prevent the use of non-miraculous treatment, the average value of which is greater. Not one of the "scrofulous" persons who were touched by a mediaeval French or English monarch was thereby estopped from or delayed in the application of non-miraculous treatment which was likely to relieve or cure the disability. Some derived benefit, some were harmed, others were unaffected. Professor Bloch's statistics do not enable us to make a classification, so the pragmatic value of the royal miracle cannot be determined.

England and Wales.

FORBES FRASER HOSPITAL, BATH.

DESCRIPTIONS of the Forbes Fraser Hospital at Combe Park, Bath, were given in our issues of May 24th and July 26th, 1924. At the last meeting of the hospital committee the chairman, Sir Percy Stothert, was warmly thanked for a gift of £675, the cost of the new retiring rooms for the nurses. A discussion took place regarding the site of the new nurses' hostel which is to be built in connexion with the hospital, and towards which Mr. Stanley Wills recently gave £5,000, and a letter from Sir George Wills was read, enclosing one hundred guineas towards the cost of the equipment of the hostel. The Mayor of Bath, Alderman Cedric Chivers, thereupon offered to present to the committee a site of some four acres adjoining the hospital, and this was gratefully accepted. In the course of the meeting it was mentioned that the Forbes Fraser Memorial Fund now amounted to over £2,400, and that Mr. Walter Mallett's contributions towards the Combe Park Children's Orthopaedic Hospital amounted to £4,400.

THE COST OF SANATORIUM TREATMENT.

The Ministry of Health has issued a memorandum (94/T) containing details of the expenses incurred by 173 hospitals and sanatoriums throughout England and Wales in the treatment of tuberculosis. Included in the data, which are classified under thirty-eight separate heads, are such items as the number of beds, the proportion occupied, the type of cases treated, the salaries of the staff, the amount spent on different kinds of foodstuffs, the structural and laundry expenses, and the rents, rates, and taxes. Adding these together, and deducting the profits made on the farm and garden, and sources of income other than those derived directly from the patients, we have the average weekly cost per patient. Dealing with those institutions which cater for adults only, one cannot fail to be struck by the differences presented by the individual institutions; for instance, Black Notley Sanatorium, Essex, shows a figure of 27s., whereas Peel Hall Hospital, Lancs, shows one of 69s. a week. It is clearly unfair to draw comparisons without intimate knowledge of all the factors concerned, but there seems to be little doubt that a difference such as this can hardly be ascribed entirely to local circumstances. It is interesting, too, to note that the expenses of the county council bodies agree very closely with those of the voluntary ones. On working out the average for forty of the former class we find the cost to be 49s., and for thirty-one of the latter class 48s. a week. In most cases where figures are given both for 1923 and 1924 there is seen to be a reduction in the expenditure for the current year; part of this can be ascribed to the diminished cost of food, especially of meat. Finally, attention may be drawn to the variations presented in the proportion of beds occupied to the number provided; in the majority of institutions, it is true, this proportion is high, but there is quite a number in which it does not reach above 75 per cent. The Ministry evidently considers this as an argument in favour of the free interchange of patients between one area and another. This document may be commended to the consideration of all interested in the administrative treatment of tuberculosis.

SHEFFIELD LOCAL MEDICAL ADVISORY COMMITTEE.

An event of considerable importance in medical organization is the establishment, recently, of a Local Medical Advisory Committee for the City of Sheffield. The steps which have led up to the appointment of this committee may be of interest. During the spring of this year a severe outbreak of epidemic encephalitis occurred in the district, and it was felt that an investigation of the after-effects of the disease, carried through by the co-operation of the hospital staffs and general practitioners with the health office, could not fail to be of value. Accordingly, on the suggestion of Dr. F. E. Wynne, medical officer of health, and Professor A. J. Hall, a meeting representing all branches of the profession was called by the Sheffield Division of the British Medical Association with the object of setting up a committee for the purpose of the proposed investigation. At this meeting a wider scheme was put forward—namely, the formation of a permanent advisory committee, whose advice and assistance would be available to the health authority, either in the event of other epidemic outbreaks or in any matters affecting the public health; such committee could also co-operate with the medical officer of health in obtaining facts as to the incidence of disease. There was found to be a strong feeling that the time was opportune for the institution of such a body, and the committee has now been formed. It has already got to work, and a sub-committee has been appointed to undertake the investigation of the after-effects of epidemic encephalitis. It is proposed at an early date to appoint a further subcommittee to investigate some questions concerning the incidence and effects of treatment of cancer of the breast. It is believed that the co-operation of this new committee will be welcomed by the local health authority, and also that as a step in the direction of preventive measures its objects will appeal to all branches of the profession.

MEDICAL WOMEN'S PRESENTATION TO DR. E. W. HOPE.

At a special meeting of the Liverpool Branch of the Federation of Medical Women, held on December 3rd, an inscribed antique silver tray was presented to Dr. E. W. Hope on his retirement from the post of medical officer of health for the city and port of Liverpool. Miss Margaret Joyce, president of the branch, was in the chair, and there were forty-five women doctors and students present. Miss Joyce, welcoming Dr. Hope, said she had recently heard him eulogized for the various things that he had accomplished, but the medical women wished especially to show their appreciation of what he had done for the poorer women of Liverpool. Miss M. H. F. Ivens (President of the Federation of Medical Women), in making the presentation, laid stress upon the great work done by Dr. Hope in improving the conditions of midwifery practice in the city, and his sympathetic co-operation in all efforts for the relief of suffering. She thanked Dr. Hope for his generous outlook and broad views with regard to medical women: in his whole-hearted support of their work he had shown vision and imagination. Other speakers were Dr. Vera Foley, Dr. Chambers (representing the school medical officers), and Miss Beryl Chamber (representing the women medical students). Dr. Hope, in acknowledgement, said he had belonged to the Liverpool Medical Institution for the last thirty years, and had supported the application for membership of the first woman doctor in Liverpool. He referred to women's work in public health, and mentioned that the reduction in infant mortality synchronized with the entrance of women into the public health service. In Liverpool to-day the greater part of the maternity and child welfare work was performed by women doctors, and he hoped that the number of women engaged in this work would be largely increased all over the country. Dr. Hope was accompanied by Mrs. Hope and his two daughters.

BETHLEM ROYAL HOSPITAL.

A course of lectures and practical instruction for the diploma in psychological medicine commences at the Bethlem Royal Hospital on January 5th, 1925. This course is comprehensive, and fulfils the requirements of the Universities of London, Cambridge, Durham, Leeds, and Manchester, and the English Conjoint Board. The first part

includes lectures on the anatomy, histology, and physiology of the nervous system; lectures on the psychology of the emotions, including the related questions of psychopathology and psychotherapy; lectures on general psychology, and eight sessions on experimental psychology. The second part, dealing with neurology, includes clinical demonstrations upon selected cases of nervous disease; eight lectures on neurological diagnosis; eight lectures on diseases of the brain and spinal cord; and three lectures on the *post-mortem* examination of the central nervous system. All these lectures will be illustrated by clinical demonstrations, and, in addition, demonstrations will be given on the morbid anatomy and morbid histology of the nervous system, including the preparation and staining of sections. The second part of the course also includes lectures and clinical demonstrations on psychological medicine in all its branches, and a special course of twelve lectures on mental deficiency, with clinical demonstrations. The fee for the full course is 15 guineas, and either part may be taken separately for a fee of 10 guineas. Entrants for the course who pay a composition fee of 15 guineas may, by giving notice beforehand, attend either part and postpone the other until the next session. Those who take the complete course may attend the general clinical practice of the hospital on payment of 5 guineas for six months, or 10 guineas for one year. Further details of the course may be obtained from the Physician Superintendent, Bethlem Royal Hospital, S.E.1.

Scotland.

University of Edinburgh: Extension and Benefactors.

The Prince of Wales, on December 3rd, performed the opening ceremony at the King's Buildings, which form the Chemical Department of the University, in West Mains Road, near Blackford Hill, Edinburgh. The foundation stone of these buildings had been laid by the King four years ago. The Prince, in declaring the buildings open, said that during the past ten years there had been in commercial, industrial, and professional circles an ever-growing demand for recruits who have had a competent training in one or other of the branches of science. It was to the credit of the governing bodies of the University that they had wisely embarked on new schemes of building and enlargement which would in the near future provide their students with the facilities for scientific instruction that the needs of the country and their own individual inclinations called for so imperatively. The King's Buildings were now completed in all material details, and incomplete in one respect only—the debt which the University had unavoidably incurred in carrying out this scheme, and which had not yet been paid off. Continuing, he said that Scotsmen were famous for their power of looking ahead and anticipating the future, and this building was in a sense a monument to this national gift designed to meet in advance the needs of a new age. He appealed to Scotsmen to mark their appreciation of it by helping to remove from the shoulders of the University of Edinburgh the burden which it now bore. An honorary degree was afterwards conferred upon his Royal Highness in the McEwan Hall, the Earl of Balfour, Chancellor of the University, presiding. At a luncheon held later in the Old University Library Principal Sir Alfred Ewing announced the receipt of several gifts from friends of the University. One anonymous donor interested in chemical manufactures had presented £2,500; others, on being approached, had promised respectively £1,000, £1,500, and £5,000. These, together with smaller amounts, had brought up the list of contributions to a total of £10,600. Further, the Carnegie Trust of the Scottish Universities had decided to make a grant out of its surplus to the universities, and the share to the University of Edinburgh would be something like £15,000. Recently a much larger gift had been made by Sir Alexander Grant, whose name was associated last year with the gift of £10,000 for the purpose of a Scottish National Library. The gift towards the completion of the King's Buildings amounted to £50,000. Altogether the amount, in round figures, presented in connexion with the opening of these buildings, towards their completion, was rather more than £75,000.

The King's Buildings stand on a piece of land some 115 acres in extent situated to the south of the city, and intended to provide sites for all the scientific departments of the University. Already the Departments of Chemistry and Geology are located there. The chemistry building has a frontage of 220 feet and a depth of 320 feet. The front block is of two stories, and from it run southwards three parallel corridors, off which open the main laboratories and lecture rooms with their subsidiary offices and service rooms. These are one story in height, and are lit on the factory-shed principle by a glass roof with north-slope window lights. On the ground floor of the front block are the physico-chemical laboratories, the dark rooms, and special rooms for gas analysis, etc. On the upper floor are the library, demonstration rooms, staff common room, research rooms and laboratories designed for special forms of work. The main laboratories are four in number, each 45 by 75 feet internal measurement, and fitted with benches to accommodate 288 students in the elementary laboratory, 144 students in the laboratory of the second year, 48 students in the advanced inorganic laboratory, and 40 students in the advanced organic laboratory. The physico-chemical rooms further provide working places for about 30 students. Thus there is accommodation for 550 ordinary students, and the special research rooms at present equipped afford accommodation for 25 post-graduate workers, a number capable of being doubled when all the rooms are completed. A section of the department is devoted to technical chemistry, the object being to familiarize students with technical plant and processes. There are four lecture rooms, two seated for 180 students and two similar rooms for 100 and 50 respectively. These are lit by cupolas, and three of them can be darkened by lowering the false ceiling operated by electricity. The original estimate for the cost of the department was £250,000, but the actual cost of the buildings so far constructed, inclusive of scientific equipment and all other charges, has been somewhat less than £200,000.

War Memorial to Glasgow R.A.M.C.

A memorial tablet in Glasgow Cathedral was unveiled on November 30th by Lieut.-General Sir William Leishman, F.R.S., Director-General of Army Medical Services. The memorial is for the men of the following Glasgow units in the R.A.M.C. Territorial Force who fell in the war: Lowland Mounted Brigade Field Ambulance; 1st and 2nd Lowland Field Ambulances; Lowland Divisional Casualty Clearing Station; and the 3rd and 4th Scottish General Hospitals. The tablet consists of bronze plates set in white marble, and was designed by Mr. G. M. Brand, a Glasgow architect: it has been erected on the south wall of the Cathedral nave. The ceremony was attended by a large gathering of past and present members of the R.A.M.C., as well as relatives and friends of the fallen. Sir William Leishman in his address said that the Glasgow medical units had good reason to be proud of their war record, and no words of his could do full justice to the gallantry and endurance of the officers and men under most trying circumstances. The memorial was dedicated by Dr. Lauchlan Maclean Watt, Minister of the Cathedral, and afterwards wreaths were placed by Colonel Sir G. T. Beatson, K.C.B., honorary colonel R.A.M.C., 52nd (Lowland) Division, and Colonel G. H. Edington, A.D.M.S. 52nd (Lowland) Division. Pipers subsequently played "Lochaber No More," and buglers sounded the "Last Post" and "Réveillé."

New Hospital for Sick Children at Aberdeen.

At a meeting of the town council of Aberdeen on December 1st a letter was submitted from the directors of the Royal Aberdeen Hospital for Sick Children representing the urgency for the erection of a new hospital, and requesting the town council to sell to the directors a portion of the land which it was proposed to acquire in connexion with the joint hospital accommodation scheme. The directors of the Sick Children's Hospital stated that they

were impelled by circumstances to proceed independently, although they did not depart from the view that a joint hospitals scheme on the lines already suggested would be in the best interests of all the hospitals concerned. The town council agreed by 25 votes to 8 that the ground should be exposed for public sale for the purpose of being used as a site for the new hospital. The purchase price proposed is £5,500.

COMPENSATION AND INSURANCE RIGHTS OF THE TRAINED NURSE.

An important legal decision affecting the medical and nursing professions was delivered this month by the First Division of the Court of Session. It arose out of an arbitration under the Workmen's Compensation Acts, between a trained nurse and a lady who employed her. Apparently this is the first instance in which a qualified nurse employed on a private case has claimed to be a "workman" in domestic employment. The facts of the case were that a lady, who took a friendly interest in a neighbouring family, provided a trained nurse at her own expense to attend to one of the children. There was difficulty in getting the child to take food, and, on one occasion, the child spat out some of the food, which lodged in the nurse's eye, causing inflammation and permanent diminution of visual power in that eye. The nurse thereupon intimated a claim under the Workmen's Compensation Acts against the lady who had employed her. It was argued for the nurse that a direct contract for employment existed between the lady and herself and that, although in the performance of her professional work she was subject only to the authority of the doctor, he was in this case the agent of the lady. It was argued on the other side that the nurse was a professional person who rendered skilled services; that the nurse, further, was in no respect under the direction or control of the lady; and that the directions of the doctor were exercised in respect of his professional opinion, and not as the agent of the employer. In a lower court, the sheriff substitute had held that the nurse was a workman within the meaning of the Workmen's Compensation Acts, but the higher court on appeal reversed that decision, and the judgement delivered by their lordships is sufficiently wide to settle questions of great public importance. The decision of the Court of Session appears to be equally applicable if the patient had been a member of the employer's household instead of a child in another family, as in the particular case tried. If the judgement of the sheriff substitute had stood, then every householder employing a nurse would have to protect himself against possible claims by insurance, and the reversal of the original decision has thrown on nurses the responsibility of insuring themselves against risk of illness arising from their work, whether they are engaged through a doctor, through an agency, or directly by the sick person or his friends. The decision does not appear to affect nurses engaged on the staff of nursing homes and institutions, because they are then subject to the control of their employers in the institution, nor, presumably, does it apply to a nurse permanently employed in attendance on a child or aged person under conditions which may bring her within the terms of the Acts. The decision raises another question of equal importance regarding the position of nurses under the National Health Insurance Acts. The question often arises whether a trained sick nurse employed in a household should produce her health insurance card for stamping. This question appears to be settled by the present decision, because the words used in the Compensation Acts to define employees coming within their scope are repeated in the similar clauses of the Insurance Acts. The result of the appeal, however, in its bearing on the Insurance Acts, appears to be that sick nurses engaged on private nursing do not come under the compulsory clauses of the National Insurance Acts. Probably such nurses have until now usually considered themselves to be within the compulsory clauses of these Acts and bound to insure. It would now appear, however, that no charge should be made against the employer for stamps in such cases. The matter is somewhat further complicated by the fact that, as regards unemployment insurance, a special exemption for "female professional nurses for the sick" was considered necessary to be inserted in one of the Amending Acts of 1922, and this suggests that in the official view such nurses would otherwise have come within the scope of the Unemployment Acts, and, consequently that they come under the Compensation and Health Insurance Acts, contrary to the recent decision of the Court of Session. Probably this decision will lead to reconsideration of the whole matter and to the necessity for framing an amending bill to define precisely the position of nurses.

INQUIRY ON HOUSING IN LEITH.

An inquiry held by the Scottish Board of Health in connexion with the promotion of the Edinburgh improvement scheme was conducted last week in the City Chambers, Edinburgh, under the chairmanship of Mr. J. L. Jack, Director of Housing to the Scottish Board of Health. The inquiry concerned a corporation scheme aiming at a large clearance of what is alleged to be slum property in North Leith, and the rehousing of most of the tenants at Lochend. Dr. Robertson, M.O.H. for Edinburgh, in giving evidence, stated that the question of the present slum clearance was the subject of a clause in the bill by which Edinburgh and Leith were amalgamated. His experience in Leith and Edinburgh had shown that the environment of the population had a very marked effect upon the power of resistance to disease. That was particularly true with regard to tuberculosis and infantile mortality. Considerable improvement could be effected, and had been effected, by slum clearance schemes. In one area, which it was proposed to demolish, conditions were altogether favourable to the development of tuberculosis. There was want of light, want of air, and want of sunshine; there were no baths or washhouses, and no facilities for the drying of clothes. In one place a four-story tenement was shut in on three sides by high buildings, which had the effect of seriously interfering with the lighting and ventilation of the houses; and there was, moreover, considerable overcrowding, so that in one instance five adults and three children were accommodated in a single apartment. He was not in favour of rehousing people on these areas, on account of the congestion that must prevail, and he favoured rehousing them on areas away from the industrial centres where they worked. Undoubtedly the whole aim and object of the present housing scheme was the scattering of the population.

Ireland.

MEDICAL REGISTRATION IN THE FREE STATE.

IN his address of welcome to the licentiates who were recently admitted to the Royal College of Surgeons in Ireland, Mr. R. C. B. Maunsell, President, referred to the difficulty which had arisen with regard to the registration of licentiates and graduates from the Irish Free State, and expressed the hope that the Governments of the Free State and of Great Britain would look at the matter in a business-like way, and grant the unanimous wish of the medical representatives who had been appointed to confer and advise. He regretted to say that that hope had not yet been fulfilled. The medical profession in the Irish Free State was in imminent danger of complete destruction. The whole question had passed into the hands of politicians, who argued about clauses and subsections of clauses in Acts of Parliament and in treaties. Unless these politicians were able to find a way out of the difficulty the life of the medical profession in Ireland and its glorious traditions would count as nothing. Separation would be accomplished, but during the process of separation the profession would bleed to death. A few days ago a message was published in the daily papers purporting to come from a Minister of the Free State Government. The import of this message was that all was well, that the medical profession need not worry, that the delay was altogether due to the fact that the authorities in Great Britain were not ready to deal with the matter, but that when they were ready it would be plane sailing. Mr. Maunsell expressed his sincere hope that this optimism would be justified, but from what he knew he

feared that it might not. His advice to newly qualified men was to have their names inscribed in the *Medical Register*; nobody knew when that *Register* might be closed, but closed it would be unless a few broad-minded statesmen appeared before it was too late. Unless a graduate or licentiate from the Irish Free State had his or her name inscribed in the general *Medical Register*, or in its Colonial section, he or she would not be entitled to practise anywhere, except in the Free State—not even on the high seas as a ship surgeon. Were this to happen, medical schools and colleges in the Free State would not have sufficient students to keep them open for a single year, and all the labours of famous physicians and surgeons of the past would be annulled. Dublin, as a medical centre, would be wiped out, and students would flock to Belfast and elsewhere.

India.

RABIES IN SOUTH INDIA.

THE annual report of Lieut.-Colonel J. W. Cornwall, director of the Pasteur Institute of Southern India, Coonoor, deals with the work from March 31st, 1923, to February 29th, 1924. The number of patients who received complete courses of antirabic treatment during the year was 1,354, a decrease of 2,021 on the previous year. This decrease was due to issue of the antirabic vaccine to Government headquarters hospitals in the Presidency, and to the Madras General Hospital, 3,692 patients being thus treated. No patient died during the treatment, two died less than fifteen days and two died more than fifteen days after the completion of treatment. The last are classed as failures, giving a failure rate of 0.29 per cent. The inquiry into the mortality due to rabies was continued during the year with the view of collecting figures to give a better indication of the value of the treatment than has hitherto been possible. A considerable number of groups of people bitten by almost certainly rabid dogs have been kept under observation for several months. Some were treated and others refused treatment. It was found that 2.8 per cent. of the persons treated succumbed to rabies, whereas 6.3 per cent. of the untreated perished. This denotes a difference of 3.5 per cent. in favour of treatment, which, in other words, saved more than half the number of those who would have died from hydrophobia if they had remained untreated. A statistical table is included in the report indicating that the risk run of death from hydrophobia, if bitten by a rabid animal which is really infective, is not less than one in three. Different strains of rabies in dogs appear to differ in their infectivity towards man. One rabid dog will infect one out of every three persons bitten, whilst another will infect none. Whatever the strain concerned the dog dies and his brain responds to all the laboratory tests for rabies. Evidence is given that the widely held belief that European races are less susceptible to infection with rabies than Asiatic races is untenable. A larger proportion of Europeans than Asiatics are bitten through clothing; they generally take immediate steps to have the wounds disinfected and cleansed, and they do not waste time before beginning treatment.

BOMBAY HOSPITALS.

The report of the Bombay Medical Relief Committee, which was appointed by the Government, was published in the *Times of India* (mail edition) for October 11th. According to that journal there are four main issues: First, the number of hospitals required; secondly, their size; thirdly, their control; and, fourthly, their finance. The Medical Relief Committee came to the conclusion that under the present system 2,056 beds are available for a population of 920,704—that is, a ratio of 2.2 beds per 1,000. In its recommendations the suggestion is made that this ratio should be increased to 3.8 per 1,000, which could be effected by making additions to the existing hospitals, providing one new hospital for Worli and Mahim, providing special hospitals for tuberculosis and venereal disease, and increasing at the same time the number of maternity homes. The report includes a warning against making any one of the hospitals unduly large, and advocates the formation of some central hospitals board to obtain co-ordination of system and management, and to prevent overlapping and waste of money. The committee urges the general public to become more interested in its hospitals, and advocates the extension of voluntary gifts and of the payment by patients for treatment. There appears to be no suggestion for providing a special hospital for children at present.

PLAGUE IN THE PUNJAB.

The Punjab Government, in view of the severe epidemic of plague which occasioned over 200,000 deaths during the first six months of the year 1924, determined to adopt special measures to prevent or mitigate a repetition of this during the next plague season. The suggestion had been made that the recurrence of plague in epidemic form this year was largely due to the continued existence of plague amongst rats during the hot weather and the occurrence of climatic conditions favourable to the rat flea during the winter. It was therefore decided to endeavour to stamp out the infection by destroying the rats during the hot weather. The Public Health Department was accordingly increased by ten qualified medical practitioners, thirteen sanitary inspectors, and various subordinates, who were engaged early in July and allotted to the districts to be dealt with. About the middle of July an intensive rat destruction campaign was commenced, involving 395 localities which were potential plague centres. Traps were distributed to 17,363 houses, smoking operations carried on in 14,711 houses, and 397,784 baits were laid in 17,271 houses. As a result of this work 71,339 rats were known to have been exterminated during July. The scheme was continued and extended during the month of August, but the details of the results are not yet available. It is expected that all the known epidemic plague centres of the Punjab will have been thoroughly cleared of rats before the commencement of the next plague season. Vigorous educational work was carried on also, and 235 lantern lectures were given by officers of the Public Health Department during July alone. The Punjab Red Cross Society has arranged to provide every district with a magic lantern and a set of plague slides, together with the standard lecture prepared by the Public Health Department.

BOMBAY RECLAMATION SCHEME.

The first report of the campaign against mosquitos in connexion with the Back Bay reclamation scheme in Bombay is published by Captain B. S. Chalam, the medical and sanitary officer in charge, in the *Indian Medical Gazette* for October (p. 481). Captain Chalam points out that the growth of Bombay comprises a series of reclamations, the first of which started in 1835, an outbreak of malaria being associated with its later stages, as was the case with some of the subsequent reclamation schemes. It was therefore felt to be necessary that precautionary measures should be taken in the present instance, and a sanitary committee, including eminent medical practitioners in Bombay, both official and non-official, was appointed to give advice. The campaign began in May, 1923, a laboratory was equipped and antimalarial gangs were organized. Four species of anopheles have been detected up to date, all of which were found to breed in the brackish water of the pools on the area being reclaimed. Close co-operation has been maintained with antimalarial operations in the neighbourhood, including those in upper Colaba, where the British troops in Bombay are housed, the barracks being actually contiguous with the reclamation area. The reclamation scheme consists in the construction of a massive sea-wall, about four miles long, the area between which and the present land margin is being gradually filled with material obtained by dredging. This material is pumped in liquid form on to the area to be reclaimed, and will be subsequently covered with dry earth. After making

ample provision for roads and open spaces two and a quarter million square yards will be available for building purposes. As the sludge is deposited a large proportion of the water in it is drained off and runs back into the sea, but a small proportion of the water is side-tracked, pools being formed and antimalarial work necessitated. The preventive work now carried on includes the systematic exploration of the whole area for mosquito larvae, the treatment of all receptacles capable of holding water, removal of waste vegetation and casual water collections, and the treating with paraffin oil, tar, or other larvicide of water that cannot be so removed. An educational campaign has been arranged in conjunction with the scheme.

Correspondence.

CANCER MORTALITY IN MENTAL INSTITUTIONS.

SIR,—May I call attention to an error which is being circulated in certain newspapers and other periodicals in regard to the mortality from cancer in mental institutions? The error arises from a misinterpretation of figures given on page 34 of the Report of the Board of Control (Lunacy and Mental Deficiency) recently published. These are quoted as implying that the cancer death rate in mental institutions is "3 per cent.," whereas amongst the general community the cancer death rate is "12.4 per cent.," and from this comparison it is alleged that support is given to a certain theory of cancer causation. A moment's reflection would show that if the last figure is really correct the general community must be hastening so rapidly towards extinction that it would be well advised to take refuge in a mental institution without delay. A glance at pages 32-34 of the report referred to, however, at once relieves us as a profession from the onus of having to recommend a course so drastic, for it is perfectly clear that the figures quoted are not death rates, but *percentages of the total deaths from all causes*—that is, of every 100 deaths occurring in mental hospitals 3 are due to cancer, whilst of every 100 deaths occurring in the general community 12.4 are due to cancer. On page 32 we have been told that for 1922 the death rate from all causes per 1,000 living in mental hospitals was 90.0, as compared with 13.6 in the general community aged 15 and upwards, the general mortality rate in the institutions being, therefore, six or seven times that for the country as a whole. The actual death rates from ten important causes are then compared on the same basis in a table on page 33, from which it is seen that the crude death rate from cancer per 1,000 living in mental hospitals was 2.7, as compared with 1.7 in the general community aged 15 and upwards.

Several important factors are, of course, not taken into account in such a comparison. For example: (1) a considerable proportion of patients admitted to the mental hospitals die comparatively young from causes directly connected with the mental disorder for which they were admitted, and the population at risk to cancer and other diseases requires correction on this account; (2) the much higher mortality from tuberculosis in the mental hospitals, operating chiefly at ages before cancer comes into play, likewise reduces the population really at risk to cancer. From the information given in the report the exact effects of these and other disturbing factors cannot be computed, but it seems evident that they would mostly tend to make the apparent crude death rate from cancer in the mental hospitals too low, and that the rate, when corrected to be comparable with that given for the general community, would be considerably *above* 2.7. If we content ourselves with the crude figures as they stand, however, and assume that mental hospital inmates are indeed saved, as stated, from most of the factors which are alleged to be the causes of cancer in the articles referred to, the greater cancer death rate per 1,000 living in these institutions provides a direct contradiction to the theory advocated.—I am, etc.,

PERCY STOCKS, M.D.

Department of Applied Statistics,
University College, London, Dec. 4th.

EXPERIMENTAL CANCER IN THE GALL BLADDER OF GUINEA-PIGS.

SIR,—The pulmonary lesions to which Dr. Creighton refers (December 6th, p. 1079) are not uncommon in rats; they sometimes occur in rabbits, and, though I have not looked for them in guinea-pigs, I have no doubt they are to be found in them also. I do not think that anyone who has had experience of them regards them as true tumours, much less cancers. They bear no resemblance to the lesions we produced in the gall bladder.

Dr. Creighton says of my experiments that "it is clear that the tracts of fibrous callosity preceded the epithelial formations." In point of fact it was not so.

"Large nuclei and deep chromatization" are not, by any means, invariable characteristics of malignant cells; but if Dr. Creighton cares to insist on these features as criteria of true cancer his doubts may be removed by the statement that both were present in the section, for example, from which Fig. 21 was taken.—I am, etc.,

ARCHIBALD LEITCH.

Cancer Hospital Research Institute,
London, S.W., Dec. 6th.

EXERCISES FOR THE PELVIC FLOOR MUSCLES IN DYSCHEZIA AND RECTAL AND UTERINE PROLAPSE.

SIR,—It is a curious fact that those who are specially interested in remedial exercises entirely ignore the extremely important muscles which form the pelvic floor. I was therefore very glad to see the article by Dr. T. Stacey Wilson, on the "Cure of uterine prolapse by re-education of the muscles constricting the vagina," in the JOURNAL of December 6th (p. 1041). Fifteen years' experience has proved to me the great value of the same exercises in the treatment of the very common cases of constipation depending upon dyschezia or inefficient defaecation. I described this treatment in the first edition of my book on *Constipation and Allied Intestinal Disorders* in 1909, my attention having been drawn to the subject shortly before by a monograph written by Pincus of Danzig.[1]

I have seen several cases of rectal prolapse cured in the same way. I was, for instance, consulted in June, 1915, by a lady who had had an operation performed twelve months before for a very large prolapse, but the condition returned almost at once. She had been advised by a distinguished rectal surgeon to have another operation for its relief. Instead of this she succeeded in restoring the normal activity of the muscles of her pelvic floor by exercises. There has been no recurrence in the nine and a half years which have since elapsed, in spite of a very long and severe illness, which resulted for a time in great emaciation.—I am, etc.,

ARTHUR F. HURST.

New Lodge Clinic, Windsor Forest,
Berks, Dec. 9th.

SIR,—Dr. T. Stacey Wilson's article is most interesting and the thesis, I think, most sound. Many women cultivate—from feelings of modesty—undue passivity in regard to their sexual feelings; this passivity, long continued, leads to loss of tone in the pelvic muscles. I am of opinion that the same conclusion holds good in the case of constipation. The public indulge in laxatives which evacuate the bowel without any muscular exertion; consequently there is loss of muscular tone and bad habits are originated. I contributed a short note on this cause of constipation a few years ago, which you, Sir, did not think worth publication.—I am, etc.,

A. ADAMS.

Derby, Dec. 8th.

TONSILLECTOMY.

SIR,—As the proposer of the resolution carried by the Laryngological Section at Bradford, which has given rise to considerable discussion in the JOURNAL, I would like to make it quite clear that method of operation was ruled out by the terms of reference in the discussion of the treatment of chronic tonsillitis.

[1] L. Pincus: *Constipatio muscularis s. traumatica mulieris chronica* (Volkmann's *Sammlung klinische Vorträge*, 474, 475), 1908.

From the papers read at that discussion it seemed quite clear that the tonsil should be regarded as having no special function apart from that of a limited barrier against infection, that this barrier became not only useless but a source of danger when the tonsil was chronically infected, and that when surgical procedures became necessary total extirpation of the tonsil should invariably be done; no methods of achieving this were discussed.

The terminology of the resolution "Tonsillectomy by enucleation" was possibly unfortunate, as giving rise to misunderstanding, the words "by enucleation" being redundant, but inserted in order to emphasize the word "tonsillectomy." It was felt that so many cases were quoted of patients having had tonsils "removed" two, three, or more times, that it was necessary to emphasize the word "tonsillectomy" in order to prevent, as far as possible, in the future patients being subjected to what everyone must consider a lamentable redundancy of operations.

That tonsillectomy by dissection should be invariably performed was never hinted at, nor did the meeting understand the resolution in this sense; but it was felt that, whatever the method of operation, it should be undertaken under strict surgical conditions, not haphazard; hence the second resolution regarding the classification of the operation.—I am, etc.,

Southsea, Dec. 8th. C. A. SCOTT RIDOUT.

SIR,—The discussion on this subject in your columns is very interesting, but it does not seem to be making many converts. Indeed, it would be strange if it were otherwise, for we have, on the one hand, a large number of doctors who have removed adenoids and partially removed tonsils, and rightly claim great benefit to their patients; while, on the other hand, we have a small body of specialists who say that nothing short of complete removal is enough.

Both parties are right in their contention, but unfortunately, as is often the case in these prolonged discussions, both parties are not talking about the same thing. The tonsillotomists are talking about the results of operations upon so-called tonsils and adenoids, whereas the tonsillectomists are talking of diseased tonsils only, and there is a vast difference between the two.

Adenoids produce the following group of symptoms: nasal obstruction and, in consequence, mouth-breathing, nasal catarrh and nasal sinus disease, pharyngitis, laryngitis, and broncho-pulmonary disorders. They produce nearly all the diseases of the ear of childhood, and a proportion of those of adult life. They also cause enlarged glands in the posterior triangles of the neck. This is a comprehensive list—and one poor child may have them all—so that it is not surprising that removal of adenoids is of great benefit to the patient.

Tonsils have a far worse reputation, but they bear the blame for many of the crimes committed by their neighbours, the adenoids. They produce enlarged glands in the anterior cervical triangle. They produce a favourable site for many of the infectious diseases, and, when chronically infected, are the foci from which arise many system toxaemias and infections which undermine the health and efficiency of patients of all ages.

On comparing these two groups of symptoms it will be noted that adenoid symptoms are more noticeable and dramatic; furthermore, the mouth-breathing produced by adenoids aggravates if it does not produce the enlargement of the tonsils. Hence, on removing the adenoids the patient is relieved from the symptoms produced by them, and the tonsils tend, if not too far gone, to return to normal.

It is the careless combination of the symptoms produced by tonsils and adenoids which is responsible for the varied opinions on this question of tonsillectomy. I am convinced that, when children are cured by removal of adenoids and partial removal of tonsils, it is the adenoids which are producing the symptoms, and that the same result would be attained were the tonsils left untouched. It has been proved that a diseased tonsil is diseased right down to the capsule, so that mere tonsillotomy cannot possibly eradicate the disease. As it is impossible in many cases to differentiate between the symptoms produced by diseased tonsils and adenoids the safest plan is to enucleate the tonsils. It is, in skilled hands, just as safe an operation as tonsillotomy, and those who have not the necessary skill should acquire it. It is no excuse to say that there are not enough skilled men in the country to cope with the work. There never will be enough if doctors will not take the trouble to learn.—I am, etc.,

Southampton, Nov. 28th. W. STEWART.

SIR,—Mr. Cecil A. Joll's letter (BRITISH MEDICAL JOURNAL, November 29th, p. 1025) opens up the question of anaesthetic technique from a new point of view. He indicts the anaesthetists because in their ranks he thinks there are not enough sufficiently trained. This training, in his view, means that the anaesthetist has such easy command of the patient that he can divide his attention between the anaesthesia and the operation. In practice this may sometimes obtain, but in theory it is absolutely wrong. Mr. Joll, although he has given up the rapid guillotine method for the method of dissection, has, it would appear, failed to appreciate the radical difference in the anaesthesia required.

In the old days, when the guillotine method was universal, all that was required was that the patient be deeply under and the operation would be completed without the continuance of anaesthetic agents. Then the anaesthetist could quite easily give as much operative assistance as the surgeon needed. But with the advent of the enucleation method, hurry and rush have been entirely superseded by the quiet and deliberate routine of a major operation. In this there is usually very little haemorrhage; all the surgeon requires of the anaesthetist is that he may proceed at his own pace. He packs the tonsillar fossae himself, and why shouldn't he? There is plenty of time; for the sake of argument, it matters not if the operation takes ten minutes or half an hour.—I am, etc.,

BERNARD E. POTTER.

London, W.1, Dec. 4th.

SIR,—Now that tonsil warfare has once more broken out in the JOURNAL, and before you yourself cry "enough" and apply the guillotine, I would suggest that one matter-of-fact aspect of the case, which helps to decide the surgeon's choice, has been rather overlooked.

In actual practice, when the operator has to deal with a number of tonsil enucleations in a limited time and under short anaesthesia, as is often the case, the so-called Sluder's way with the guillotine is clearly indicated, and many have now acquired by practice great skill in this method. When, however, time does not press so much, and with difficult cases and longer anaesthesia, most operators will prefer dissection. I refer, of course, to general anaesthesia; local anaesthesia has its advantages, but has not so far been generally accepted as suitable for the operation of tonsillectomy.—I am, etc.,

London, W.1, Dec. 8th. W. H. KELSON.

THE GENERAL PRACTITIONER AND THE MATERNITY SERVICE.

SIR,—The discussion on puerperal fever which took place at the Royal Society of Medicine was strictly limited to the definition of the condition and the means and manner of notification, so that there was no opportunity to discuss the relative responsibility of the general practitioner; but there can be no doubt that there is an opinion abroad, especially in official quarters, that he is answerable for an unduly large share of the existing puerperal mortality and morbidity.

His proportionate responsibility cannot be determined until we know the results of the work of the specialists in their private practices and of the various institutions which they serve, though it is doubtful if properly comparable figures can ever be obtained. Probably all are responsible for some mistakes, but there is one fact that should not be overlooked: a very small proportion of careless practitioners will vitiate the statistics of all concerned, and bring unmerited blame on the great majority.

What are the charges brought against us in particular—

are they well established, are we alone to blame for them? The evidence is said to be found in the statistics of maternal mortality, which in twenty-three years has only dropped from 4.81 to 3.60 per 1,000 births. If we examine these rates we find that "sepsis" has gone down from 2.18 to 1.30—that is, by 0.88—whereas "other causes" has dropped from 2.63 to 2.30, which is only 0.33; so that practically three-quarters of the total reduction of 1.21 is due to the decrease of that sepsis with the production of which we are charged—that sepsis which is to-day not much more than half what it was twenty-three years ago. As to "other causes," it is admitted that some of these are, with our present knowledge, inevitable, and a considerable proportion of them must be due to the survival of the unfit child-bearers whose lives have been saved by the science and art of the best midwifery.

It has been stated that after allowing for the cases in which the general practitioner has been called in by the midwife, he still has the larger ratio of septic cases; but it must be remembered that a large proportion of his own patients are primiparae and others who, for one reason or another, anticipate some difficulty.

Although all will admit that faulty technique may be the most usual cause of puerperal sepsis, there can be little doubt that in many cases there must be other factors of the exact nature of which we are ignorant—conditions of the patient, both genital and extragenital, over which we cannot as yet exercise any control—just as some persons are liable to severe attacks of furunculosis or erysipelas without any obvious cause. These puerperal fevers may all be directly due to three or four generally recognized bacteria, but that does not necessarily imply that they have gained access to the patient and overcome her resistance by the fault of her attendant.

The practitioner is also often put at a great disadvantage, both as regards sepsis and other complications, because he is called in, too late, to remedy some condition that, had it been recognized before, might then have been dealt with successfully. Until it is generally recognized, especially by the patients themselves, that the actual confinement is only one stage of the whole process of pregnancy, there seems little chance of this risk being done away with.

The family doctor is said to show want of care in his technique, especially as regards asepsis, undue interference and haste, and to be ill informed in his art. If these statements are correct, is he chiefly to blame? His education was what was given him, not what he asked, and after he was in practice he was often confused by changing and even opposing obstetric dogmas. The conditions under which he has to do much of his work are such that I doubt whether any specialist would accept them—surroundings too often unsuitable, nothing clean, everything overcrowded, inefficient help, interfering relations and friends. Time—and this is all-important—is against him. He cannot choose the moment, it is too frequently wasted by doing the work of the nurse, by watching that should be done by the midwife; it may interfere with his other work and cause an actual loss of income. The remedy for these things is not his; it rests with those who should provide better homes, proper institutions within reach of the practitioner, trained nurses or midwives to help, and fees proportioned to the skill, the time, the energy, the equipment required, and the responsibility entailed.

And is the medical man, whose greatest encouragement to good endeavour is success in the practice of his art, likely to be interested in work that is so often disheartening, where time may have to be wasted, energy thrown away, trouble ill requited, and responsibility unfairly weighted? When one considers what the difficulties and discouragements have been, one can only wonder at the profound change which, in spite of all, has been wrought in general practitioner midwifery during the last forty years.

If all concerned will make every possible endeavour to remove the causes that contribute directly to the diseases and disasters of midwifery, if they will make their prevention possible, general practitioners will, instead of letting themselves be displaced by midwives and specialists, be able and glad to take, with them, their proper place in the service for which they have done so much in the past, and will, if allowed, do still more in the future.—I am, etc.,

Bradford-on-Avon, Nov. 30th. CHAS. E. S. FLEMMING.

SIR,—I am afraid your appeal to the general practitioners to ventilate their views on the present practice of obstetrics is not likely to result in much good. I cannot but think that the subject has been approached in a wrong way. Nearly every correspondent refers to the "storm now raging," "the dispute," "the century-old controversy," whether it be between the midwife and the general practitioner or the consultant, or all three, and proceeds to add fuel to the flames.

Why not recognize the fact that maternal mortality remains sadly too high, under the present condition of the service, and endeavour to improve matters? In a previous letter (November 22nd) I briefly indicated how this, in my opinion, might be done, and I would like to pursue my suggestion—namely, that every case of confinement be seen at least once by a medical practitioner—a little further. Our aim should be to anticipate complications that may threaten, to recognize the conditions predisposing to sepsis, abnormal presentations, contracted pelvis, or any condition of the patient herself, such as cardiac disease or albuminuria. The point is how to get the public to recognize the necessity for such examination. Well, in these days, many women engage a midwife only to attend them, or only provisionally engage a doctor as well. Why not make it *compulsory* that such contracts between patient and midwife be made in writing (or printed), in proper legal form, signed by both parties, and contain a clause that a doctor be consulted at least once, the patient being responsible for the fee? At what period such a consultation should take place would be a matter of opinion, and could be left to the patient and midwife to decide.

In this connexion I consider it most essential that every area should have the services of one or more fully qualified and trained midwives available, and doctors should see to it that such are made full use of. In some such way the services of both doctor and nurse would be co-ordinated and made interdependent and not antagonistic. The doctor would know in time what cases would be best removed to a maternity home or other institution, and what cases would require further attendance in their own homes. The advantage of such a scheme as this lies in that it could be put in operation at an early date. Time would show whether the better results were obtained by institutional treatment or having the patient in her own home. In the meanwhile, it is the business of each local authority to see that arrangements are made to provide proper accommodation and means of transport for cases requiring institutional treatment and the business of the training and licensing boards to see to it that the standard of the whole maternity service is improved.—I am, etc.,

Wymondham, Norfolk, Nov. 29th. ALEX. P. AGNEW.

SIR,—I have been interested and rather amused at the letters about institution treatment for maternity cases. Haven't we poor general practitioners already enough notifying and inspections without adding to our burdens? It is forty years since I left hospital, and for the whole of that time I have been engaged in private practice. I have attended at over three thousand confinements and have never lost a mother. I do not think any amount of institution treatment would have improved on this, and I am certain that there are hundreds of general practitioners who can boast of the same results.

I have had to treat every complication conceivable, and have, with the help of a brother practitioner or assistant, managed them all—except a Caesarean case that had had much ante-natal care and measurements done in London, and when labour was fairly advanced I found a projection from the sacro-lumbar junction prevented delivery. My knowledge of midwifery when I left hospital was beneath contempt—twelve "B.B.A.s," seven normal labours, and watching our resident obstetric officer extract with forceps

once. I was fortunate in my first practice to get help and instruction from one of the best men who ever wielded a pair of forceps.

Why this outcry about forceps? They have saved many a perineum, many hours of agony, and many a child's life. A primipara with a dose of scopolamine-morphine, chloroform, and forceps can spend her last four hours or so in a happy oblivion, instead of having an experience that will remain in her memory for a lifetime.

Is not sentiment to have any part in the last months of pregnancy of the young mother? Is she not to have the doctor of her choice, in whom she has confidence (who may even have brought her into this world), the rapture of showing father the little darling almost as soon as it is born, the comfort of her own home, and the support of admiring relatives and friends? Instead of this—frequent examination, packed off to an institution, labelled by a number.

How many confinements has the ordinary specialist attended? Would he do a Caesarean section for every occipito-posterior case? I have never yet failed to extract with forceps; and these cases are pretty numerous, and getting still more so as woman is losing her pelvic curve and getting flatter—due, in my opinion, to too much athletics.

No; leave us our freedom, our pain alleviatives, and, above all, our forceps, and the results will not be so disastrous as some of the faddists think. But give the student, and make him take, more opportunities of perfecting himself in the practice of midwifery. A man who can really use forceps, do a podalic version, and arrest haemorrhage in placenta praevia—all of which are simplicity itself to the average general practitioner—can be safely left to attend 99 per cent. of the cases likely to come under his care.—I am, etc.,

Frinton-on-Sea, Dec. 1st. G. CRAIGIE BELL.

PUERPERAL SEPSIS.

SIR,—Recent articles and correspondence recall to mind an epidemic of puerperal fever which fell upon us in Mid-Glamorgan in 1891, with numerous fatalities among the primiparae, of whom there were many in the new and growing colliery villages.

In the centre valley there were three practices, and together we must have lost at least a score of young mothers in the short space of a month. There were four deaths in my practice in less than a fortnight and in no case was I present at the birth. There were three midwives in the village, and they were as capable as most of their kind. The fatal cases were impartially distributed amongst them, the labours being easy in each case. During the same period a colleague in the same village lost at least an equal number of cases, also primiparae, and he also had not been present at any one of the births.

The invariable history was a rigor on the fourth or fifth day (and at this period in the case we were called in), quickly followed by vomiting which soon become more and more bilious in character until the fatal issue. As an Irish assistant tersely put it, "That green vomit is the very divil." Loss of consciousness occurred only in the final agony, which was not many days coming. In no case was there marked enlargement of the uterus and tenderness was not complained of on pressure. The discharges were not increased or foul-smelling.

The valleys on either side suffered in much the same way; in fact, one of my expectant patients fled in terror to some friends who lived five miles away, only to fall a victim when her first baby was born. She fortunately escaped with her life, but with the loss of her baby.

During the same weeks multiparae—even severe instrumental cases—went through their trouble without any disturbing incident.

As a preventive measure the midwives were warned off their work, but this did not stay the epidemic. The county medical officer came and made a painstaking inquiry as to causation; but his investigation did not add much to our knowledge or comfort. The great epidemic of influenza was gradually dying out, and the vagaries of this strange disease were, I must confess, more than suggested as possible causes.

One interesting fact must be recorded. Scalp wounds and other colliery scars, which usually healed by first intention, at this time got into the bad habit of suppurating, reminding one of old days in the "surgery" at Bart's, when the linseed-meal poultice was much in evidence.—I am, etc.,

Newport, Mon., Nov. 29th. J. LEWIS THOMAS.

SIR,—Your September numbers have just reached me and I am keenly interested in the discussion on puerperal sepsis. The great majority of cases which run a temperature of over 100° subside within four days and need no further treatment. Where the temperature persists either the patient's normal resistance has not been sufficient, or she has some other focus of infection which has rendered her less refractory to the organism than she normally would be. What she needs is to have her tissue resistance reinforced.

For over a quarter of a century I have successfully used antidiphtheria serum for this purpose—but it is administered orally, as the antitoxin has nothing to do with the object in view and is not absorbed when orally exhibited. The elements in the serum by which this tissue stimulation is produced are the endocrine gland secretions which have been developed in the animal to defend its tissues during the process of immunization for diphtheria. They are tissue stimuli produced in Nature's laboratory for this definite purpose. These secretions are available for the tissues in the presence of the staphylococcus and streptococcus as well as in diphtheria, and not in midwifery alone, but wherever those organisms are found in disease.

The use of antidiphtheria serum orally for septic conditions has steadily extended here, and to supply the demand for it the Commonwealth Serum Laboratory has prepared a low potency serum and listed it at 7s. per 100 c.cm. My own supplies for years came from Messrs. Burroughs Wellcome and Co., and the strength was 200 units per cubic centimetre, and the serum was satisfactory. The dosage is 30 to 100 c.cm. per diem in 10 c.cm. doses, according to the severity of the case, the larger doses only being used when the smaller fail. I shall be glad to hear that it has been successful in other hands.—I am, etc.,

Melbourne, Oct. 30th. D. MONTGOMERIE PATON.

MULE-SPINNERS' CANCER.

SIR,—I read with interest your leading article of November 22nd on mule-spinners' cancer. There is, however, at the commencement of that article a statement which is not quite correct. You write, "It is difficult to realize that only two years ago the term 'mule-spinners' cancer' was unknown." In the same number of your paper is the report of an article by Dr. A. Leitch on "Mule-spinners' cancer and mineral oils," in which there occurs the following sentence: "Strange to say, no one until within the last two years seemed to have known or to have suspected this association."

In a previous letter, which you published on December 9th, 1922, I wrote: "I noted clinically the association of the disease with mule-spinners as far back as 1906-7, whilst house-surgeon for the late Mr. G. A. Wright." The actual work was done whilst I held the Tom Jones Surgical Scholarship (1907-10), and was mentioned by Mr. John Morley in a paper on the "Lymphatics of the scrotum" (*Lancet*, December 2nd, 1911). Mr. Morley refers to the fact that cancer of the scrotum is by no means so rare in Lancashire as is generally supposed, and continues:

> "The frequency of the disease has been definitely established by the work (at present unpublished) of Mr. S. R. Wilson, who, in the course of an etiological investigation into epithelioma of the scrotum, has collected a series of 40 cases from the records of the Manchester Royal Infirmary and the Christie Cancer Hospital during the past ten years."

Purely personal reasons interfered with publication at the time, and further delay occurred owing to the intervention of the great war. I then asked Mr. Southam to bring the work up to date, and we issued a joint publication in the *British Medical Journal* of November 18th, 1922.

I feel it is only fair to the Manchester Medical School and myself to mention these facts, for Manchester surgeons have taught the association of mule-spinning and epithelioma scroti since 1910.—I am, etc.,

Manchester, Dec. 2nd. S. R. WILSON.

RAIN-BEARING WINDS AND EARLY PHTHISIS.

SIR,—My name appears in Dr. Gordon's paper in the BRITISH MEDICAL JOURNAL of November 29th (p. 983), so perhaps you will allow a few comments.

If Dr. Gordon's hypothesis is confirmed by independent observers, and rain-bearing winds are found to be the main factor in the incidence, and the most important factor in the prognosis, of phthisis, the tuberculosis problem will be at once simplified, and treatment will have to be profoundly modified. The subject merits dispassionate consideration.

My own experience in South Devon leads me to believe that Dr. Gordon is mistaken. My district contains several localities—Dartmouth, Brixham, and Salcombe, for instance—which are almost entirely sheltered from south-west and westerly winds, where yet the incidence and mortality from tuberculosis is higher than in Torquay, which is exposed for the most part to warm, rain-bearing winds but sheltered from cold winds.

For generations medical men have sent tuberculous patients from all over Great Britain to South Devon, and especially to Torquay; 40 per cent. of my notifications are importations, and one well knows that only the minority of importations are notified. It is difficult to believe that if all these patients had done badly their doctors would continue to send others.

Dr. Gordon presents us with a hypothesis based on statistics, but, it must be said, based on very malleable statistics, which lend themselves obviously to considerable subconscious moulding by an enthusiast. And even when another, perhaps apathetic, observer is co-operating with an enthusiast, the final result will not much differ from his unaided conclusions. It must be admitted that enthusiasm is infectious.

If this hypothesis is to be accepted, it will entail such important alterations in our methods that I would suggest certain steps before acceptance.

1. Dr. Gordon's chief axiom that an elevation of 100 ft. shelters half a mile should be carefully checked by accurate observations. On the face of it this seems most unlikely. Does St. Paul's Cathedral, for instance, shelter the Bank? Does a wall 10 ft. high shelter an area of 88 yards behind it? I very much doubt if 100 ft. will shelter even as much as another 100 ft.
2. The Meteorological Office might tell us the ten localities in Great Britain most exposed to rain-bearing winds and the ten most protected. The incidence and mortality from tuberculosis would then be investigated in those areas by three independent, unprejudiced observers.
3. The decision whether any spot is sheltered or not depends largely on the opinion of the observer. When houses and streets in a town are considered, the subject becomes still more nebulous. One hundred houses should be chosen and three observers asked to decide independently whether they are sheltered or exposed and their decisions compared. I doubt if there is any material difference in the air of adjoining localities when a strong wind is blowing.
4. The whole of Dr. Gordon's statistics should be carefully overhauled by independent observers.

These suggestions could easily be carried out, and we should then stand on solid ground. It is my impression that, in the great majority of cases of phthisis, an east wind is the most dangerous. Some robust patients may stand this, and improve because they have better appetites, but these form only a small minority.

The influence of winds depends on other associated factors. In a cold climate absence of wind (any wind) is important, and this is attained at Davos. In a warm climate, on the other hand, exposure to wind may be desirable. And whereas exposure in the summer (when sanatoriums are mostly visited) may be beneficial, the reverse is true in the winter. In this country, where extremes of temperature are unusual, I would regard the ideal sanatorium site as one at considerable elevation, well sheltered from the north and east, but more open to the south and west. The number of days in a year when uncomfortable and harmful heat is to be expected at a high altitude in this country is negligible.—I am, etc.,

Paignton, Nov. 29th. E. WARD, Tuberculosis Officer, South Devon.

POST-OPERATIVE PULMONARY EMBOLISM.

SIR,—The report of the discussion on post-operative pulmonary embolism in the JOURNAL of November 8th (p. 850) had a special interest for me in that whilst the discussion was taking place I was recovering from an attack of post-operative pulmonary embolism. My personal experience may therefore be of some interest and may help to guide someone in making an inquiry into this important and occasionally tragic complication of surgical work.

I have seen not a few cases where pulmonary infarction occurred after a clean operation. The symptoms were a sharp, sudden, stabbing chest pain, an area of dullness in the lung with altered breath sounds, and blood-stained sputum. Almost all these cases recovered. It was evident that the "clean" embolus was a small one. I have records of two cases where, after an "attack of piles," the patient developed an atypical "pneumonia" which finished up as an "empyema," and in these cases I have not the slightest doubt that a small infected embolus produced an infarction in which the organisms grew till the pleural cavity was invaded and the case became classified as an empyema. Drainage led to a successful issue in both cases. I do not think the thrombosed inflamed pile is sufficiently recognized as the initial lesion in cases of atypical pneumonia in adult persons where the termination of the case is a foul "empyema."

Massive thrombosis such as takes place in the lower limbs may lead to the separation of a large embolus which will almost certainly and quickly produce death, and it is this type of embolism which produces surgical catastrophes. This thrombus may, especially in pelvic cases, be septic, in the ordinary acceptance of the term, but I disagree with the view that all such thrombosis is the result of the presence in or near the vessel of pyogenic organisms or their products. There is undoubtedly an altered state of the blood, but that there is necessarily pyaemia, septicaemia, or a localized pyogenic organism as the causal factor I do not believe. The statement that every embolus, small or great, which occurs after an operation is traceable to a focus of sepsis at the site of the operation is, I consider, absurd.

Before my operation I was mentally and physically fatigued after a hard winter's work, and I had barely recovered from an "influenzal cold." However, I did not consider myself so very much below par, and in order to save time I submitted myself to a herniotomy and appendicectomy without a qualm. I was not purged; I was not starved; I did not lie on my back for more than twenty-four hours; I was out of bed on the third day, and I left the nursing home on the tenth day. None of the etiological factors, then, on which so much stress is laid are relevant to my case. All this time my temperature was normal and my pulse about 70 a minute. Nevertheless, I did not feel right. I had a little sense of "hardness" above the symphysis pubis. It crossed my mind to ask my surgeon if he had ligatured the deep epigastric vessels, but I thought little of it. At the beginning of the third week I noted that my pulse was between 90 and 100, though I was taking food well, was sleeping well, and was moving quietly about my home. Then one night after I had reached my bed all the classical symptoms appeared—the pain, the breathlessness, the palpitation, and later the blood-stained sputum. I thought at first it might be a flare-up in some residuum of my "influenzal cold," but when the symptoms recurred three, four, and five times and I was able to direct my physician's stethoscope to the site of the last stab, I concluded, with my advisers, that a series of emboli was being shot off, probably from the site of the operation.

The two biggest emboli threw the bases of the lungs out of action, but after the immediate distress of the lodgement of the embolus was passed my tachycardia was "almost miraculously" controlled by an intravenous injection of strophanthin, and the use of omnopon enabled me to sleep and to reserve my strength. My sputum, at first quite blood-stained, later became clear and frothy, but never on any occasion was purulent or muco-purulent.

For several weeks I went along slowly, with a raised temperature and a pulse of about 100, feeling toxic and gaining very little. At last I was allowed out of bed; that night

my left leg became painful (ten days previously I had had a twinge of pain in the calf), and by morning it was clear that a thrombosis was taking place. My sensations led me to conclude that a degree of thrombosis took place up the external iliac vein (I was conscious of a peculiar ballooning of the sigmoid flexure). I had once more to resort to sedatives in order to get any rest, and the whole of my leg became considerably swollen. About seventeen days later the right leg followed suit, but the process was much less severe, and after fourteen days I felt better than I had since the onset of my illness, and was shortly afterwards allowed up. Twice later small superficial veins crossing the hypogastric region became hard and uncomfortable, but in no way retarded my convalescence.

My case, therefore, was one not only of post-operative embolism by aseptic thrombi, most probably from the site of the operation, but I had a massive thrombosis, as I believe, not only of the veins of both legs, but also of the external iliac veins. No one can explain why these small emboli were not followed by much larger ones at the later date. My wounds healed perfectly and have never given the least concern. A toxin, producing thrombosis, may have been present in my blood, but there never was the least manifestation of any localized or generalized sepsis.

Like a good story-teller, I have kept the essential part of my story till the end. At my request my surgeon, whilst I was anaesthetized, removed three molar teeth which had been giving me some discomfort. Healing in the jaw took place without a twinge of pain, but I think, nevertheless, that all my illness is attributable to causes originating in the tooth sockets.

The relation of dental and oral "sepsis" to thrombosis is a matter which requires investigation. Before dismissing the subject as simply a manifestation of toxin absorption (that is, confessing that at the moment we know next to nothing about it), the question of thrombosis being the result of a blood invasion by one or other of the many spirilla which we are told infest the oral cavity ought to be settled. I once saw a demonstration of spirilla in the blood when the blood was being examined for some other reason, and I now suggest to any competent observer that in cases of non-septic thrombosis search should be made in the blood, in the clot, and in the vessel walls for the presence of some small spirillum which has hitherto escaped notice.

The lesson to operating surgeons is, of course, that no operation of choice should be performed unless the patient is in top form, and that attention to the condition of the teeth is more important than has even yet been recognized. My colleague and friend who operated on me is, of course, in no way implicated, and I entirely take to myself the total responsibility for the course of events. I am now, after eight months, in first-rate health and able to do a full day's work.—I am, etc.,

W. RANKIN,
Honorary Surgeon, Royal Hospital for Sick Children, Glasgow.

Glasgow, Nov. 12th.

Obituary.

DR. PERCY PETER JAMES STEWART, who died on November 24th, at the early age of 37, was the youngest son of Dr. Stewart of Dunedin, and was educated at the Otago University, where he graduated M.B., Ch.B. in 1912, with distinction. He served as junior house-physician and senior house-surgeon to the Dunedin Hospital, and in 1915 took the diploma of F.R.C.S.Edin., after which he held a temporary commission in the R.A.M.C. and served at the Edinburgh War Hospital for six months. He was then ordered to the East and served in Egypt, Palestine, and Mesopotamia. He was promoted to the rank of captain (acting major) and received the O.B.E. He was surgical specialist to the 75th and 32nd Casualty Clearing Stations of the Egyptian Expeditionary Force. After demobilization he joined Dr. McMaster in practice at Rugby. The cause of death was scarlet fever, contracted while attending a case. Dr. Stewart, who was a member of the Warwick and Leamington Division of the British Medical Association, is survived by his widow and two children.

We regret to record the death of Dr. PHILIP LAMBERT BENSON, which took place suddenly at his residence at Steeple Claydon, Bucks, on November 25th. He was born in Ireland in 1851, and was educated at Queen's College, Cork, graduating B.A.R.U.I. in 1870, M.A. with honours in 1871, M.D. with honours in midwifery in 1872, and M.Ch. in 1873; in the latter year he also took the diploma of L.A.H.Dub. He had practised in Steeple Claydon for about half a century. He was medical officer for the Marsh Gibbon district of the Buckingham Union for forty-one years, and had been medical officer of health to the Buckingham Rural District Council since March, 1898. Dr. Benson was a vice-chairman of the Bucks Panel Committee, and took much interest in the work of the British Medical Association, having been for several years a member of the Council of the South Midland Branch and was chairman of the Buckinghamshire Division in 1913; he was also an associate member of the Reading Division, and was president-elect of the South Midland Branch.

DR. LUCIUS GERALD ARMSTRONG MACDONNELL died on November 27th, at the age of 60. Dr. MacDonnell was the second son of the late Colonel W. E. A. MacDonnell, Deputy Lieutenant of County Clare, and of the Hon. Mrs. MacDonnell of Dublin. He was educated at Eton and Trinity College, Dublin, and obtained the degrees of B.A., M.B., B.Ch., B.A.O. in 1890, proceeding M.D. in 1900. He spent the greater part of his life in medical practice in Queensland, and was for many years the medical officer to the Herberton District Hospital, where he was distinguished for his surgical work. Although well over the military age at the outbreak of war, he was one of the first to volunteer, and was appointed to the 2nd Light Horse Field Ambulance, with which he saw active service. Later on he was regimental medical officer to the 3rd Battalion at Lone Pine, and to the 5th Light Horse Regiment at Chatham's Post. He contracted enteric fever and heart trouble and was invalided to Australia. Although the heart lesions persisted, he recovered sufficient health to undertake the command of No. 6 Australian General Hospital at Brisbane, with the rank of lieutenant-colonel. After the death of his only son in France, in July, 1918, Dr. MacDonnell came to England, and was appointed medical adviser to the Reparations Claims Department of the Board of Trade, which appointment he held at the time of his death. He leaves a widow and a daughter. A colleague writes: "He was beloved by all who knew him, and died from his devotion to duty and his loyalty to his country."

The Services.

DEATHS IN THE SERVICES.

Lieut.-Colonel Thomas Arthur Dixon, R.A.M.C. (ret.), died at Newlands, Sheringham, on November 17th, aged 71. He was born at Upleatham, Yorkshire, the second son of the late Rev. T. H. Dixon, Vicar of Upleatham; he was educated at St. Bartholomew's Hospital, and took the M.R.C.S. in 1875, and the L.R.C.S.Ed. in 1878; in 1897 he graduated M.D.St. Andrews. He entered the R.A.M.C. as surgeon in 1881, became lieutenant-colonel after twenty years' service, and retired in 1903. He was employed, on the retired list, at Maidstone from 1905 to 1908, and rejoined in 1915 for service in the recent great war.

Lieut.-Colonel Alexander William Sampey, R.A.M.C., died at Cawnpore on November 16th, aged 52. He was educated in Dublin, and took the L.R.C.S.I. and L.R.C.P.I. in 1897; he took the D.P.H. from the same source, with honours, in 1904, and the Diploma in Tropical Medicine at Liverpool in 1906. He first joined the R.A.M.C. as a civil surgeon in the beginning of 1900, took a commission as lieutenant on June 27th, 1901, became major in 1913, and received a brevet lieutenant-colonelcy, for services during the war, in 1919.

Surgeon Lieut.-Colonel Julius John Wood, Bengal Medical Service (ret.), died at Twickenham on October 29th, aged 82. He was the son of the late Rev. James Wood, Free Church minister of Dumfries, and was educated at Edinburgh, where he graduated M.B. and C.M. in 1865. He entered the I.M.S. as assistant surgeon in 1866, attained the rank of lieutenant-colonel after twenty years' service, and retired in 1892. Most of his service was spent in civil employment in Bengal, in the sanitary department, where he was deputy sanitary commissioner of the Chota Nagpur and the Darjiling circles, successively. He was the author of a botanical work, the *Plants of Chutia Nagpur*, published in 1902.

Universities and Colleges.

UNIVERSITY OF CAMBRIDGE.

Diploma in Psychological Medicine.

A COURSE of instruction covering the material necessary for the psychiatrical portion of the examinations in Part II of the Diploma in Psychological Medicine will be held in the Psychological Laboratory, Cambridge, and in the Fulbourn Mental Hospital on Tuesdays and Fridays of the Lent Term, 1925, from January 16th to March 10th inclusive. The fee for the entire course will be 8 guineas. The course will include sixteen lectures on psychopathological theory and sixteen lectures on clinical psychiatry by Dr. MacCurdy; clinical demonstrations by Drs. Reardon and MacCurdy; and instruction in forensic psychiatry, treatment, and hospital management by Dr. Reardon. Those wishing to take this course should communicate not later than December 31st with the Secretary, Committee in Psychological Medicine, Psychological Laboratory, Cambridge.

At a congregation held on December 5th the following medical degrees were conferred:

M.B., B.CHIR.—G. G. Penman, F. B. Sudbury, F. H. Mather.

UNIVERSITY OF LONDON.

IT has been decided to institute, in accordance with the regulations on university titles, a university chair of pathology, tenable at the London School of Medicine for Women, at a salary of £800 per annum. Applications to be sent to the Academic Registrar at the University by January 29th, 1925.

Dr. Joseph Bamforth and Mr. Alexander L. Urquhart have been recognized as teachers of bacteriology and morbid anatomy respectively at St. Thomas's Hospital Medical School.

Mr. S. J. Worsley has been appointed secretary to the Board of the Faculty of Medicine for 1924–25.

The following have been constituted the boards of examiners for the first and second examinations for medical degrees in 1925, the chairman of each board being indicated by an asterisk:

Inorganic Chemistry.—*Mr. J. A. Gardner (St. George's Hospital Medical School and London School of Medicine for Women) and Professor C. S. Gibson (Guy's Hospital Medical School), together with the external examiners.

General Biology.—Dr. Doris L. MacKinnon (King's College) and Dr. E. J. Salisbury (University College), together with the external examiners.

Physics.—*Mr. J. H. Brinkworth (St. Thomas's Hospital Medical School) and Mr. B. L. Worsnop (King's College), together with the external examiners.

Organic Chemistry.—*Mr. J. A. Gardner (St. George's Hospital Medical School and London School of Medicine for Women) and Professor J. C. Drummond (University College), together with the external examiners.

Anatomy.—*Professor E. Barclay-Smith (King's College) and Professor F. G. Parsons (St. Thomas's Hospital Medical School), together with the external examiners.

Physiology.—*Professor H. E. Roaf (London Hospital Medical College) and Professor Swale Vincent (Middlesex Hospital Medical School), together with the external examiners.

Pharmacology.—Dr. O. F. F. Leyton (London Hospital Medical College) and Dr. O. Inchley (King's College), together with the external examiners (*Professor A. J. Clark).

The following have been appointed associate examiners for M.B., B.S. examinations in 1925:

Medicine.—Professor F. R. Fraser, Professor F. S. Langmead, Professor H. MacLean, and Dr. Gordon M. Holmes.

Surgery.—Mr. H. S. Souttar, Mr. C. H. S. Frankau, Mr. E. D. Telford, and Professor C. A. Pannett.

Obstetric Medicine.—Mr. M. H. Phillips and Professor Louise McIlroy.

Pathology.—Dr. S. MacDonald and Dr. E. H. Kettle.

NATIONAL UNIVERSITY OF IRELAND.

DR. DENIS J. COFFEY, LL.D., President of University College, Dublin, has been elected Vice-Chancellor of the University for two years from December 31st, 1924.

The degree of M.D. has been conferred on Michael J. Horgan.

Mr. Joseph Reilly, M.A., D.Sc., F.I.C., has been appointed Professor of Chemistry at University College, Cork.

ROYAL FACULTY OF PHYSICIANS AND SURGEONS OF GLASGOW.

THE following have been admitted Fellows: H. R. Sloan, G. P. Mann.

ROYAL COLLEGE OF PHYSICIANS OF IRELAND.

AT the monthly business meeting of the President and Fellows, held on December 5th, Sir Humphry Rolleston, Bt., M.D., President of the Royal College of Physicians of London, was unanimously elected an Honorary Fellow of the College.

The following successful candidates at the winter Conjoint Final Examination of the Royal Colleges of Physicians and Surgeons, Ireland, were duly admitted licentiates in medicine and midwifery of the College:

J. J. Bourke, A. F. Cook, T. F. Crean, T. A. Cronhelm, J. Dalrymple, E. J. A. Dougan, Mary J. Fagan, E. M. Finegan, E. C. Grey, T. T. Joyce, C. Keating, M. G. Kelly, A. B. Kennedy, Wilhelmina C. Maguire, J. C. Murphy, Mary J. M'Dermott, Mary E. Nelson, T. J. O'Connor, C. R. A. O'Riordan, A. Owen-Flood, J. H. Owen-Flood, P. C. Raftery, T. C. Studeley, R. Taylor, T. W. Wilson.

Medical News.

A CONFERENCE will be held in the New Year, at the Royal Colonial Institute, to inaugurate an Empire Food Council organized to encourage the consumption in these islands of the food products of the overseas empire and of Great Britain; to spread general information about food; and to arrange a campaign to popularize bread made from wheat grown in Great Britain and the Dominions and ground in Great Britain and Ireland. The chair will be taken by Professor H. R. Kenwood, who will be supported by, among others, Sir Harry Baldwin, Sir Arthur Mayo-Robson, Sir Frederick Mott, Dr. J. E. Molson, Dr. Nathan Raw, and Dr. C. W. Saleeby. Further information can be obtained from the Honorary Secretary, Bread and Food Reform League, 37, Essex Street, London, W.C.2.

THE Lord Mayor of London presided at the annual meeting of the Royal Surgical Aid Society held at the Mansion House on December 9th. He expressed his gratification that the society at the close of fifty-two years' work was able to present so satisfactory and encouraging a report of work accomplished and public support received. The society, he said, enjoyed the continued support of the City Guilds, and its provincial branches were growing rapidly, and by the reconstruction of its premises it was able to provide improved facilities at the head office for the relief of patients. The annual report stated that during its existence the society had supplied 1,171,871 surgical appliances to 770,177 patients, of which 30,642 had been provided during the past year.

THE third annual report of the Tropical Diseases Library for the year ending July 31st, 1924, contains the announcement that the library, together with the London School of Tropical Medicine, passed on August 1st under the control of the newly constituted London School of Hygiene and Tropical Medicine (University of London). The volumes of periodicals belonging to the Tropical Diseases Bureau will remain as before on permanent loan in the library. During the past year 261 bound volumes have been added to the library, and, in addition, thirty-two old books (mostly published before 1800) on tropical diseases and naval and military hygiene have been acquired in order to form the nucleus of an historical collection. The number of those who make use of the library shows a gradual, but steady, increase.

ON his retirement from the staff of the Middlesex Hospital Dr. H. Campbell Thomson's colleagues and old fellow students entertained him to dinner at Oddenino's Restaurant on December 3rd. Eighty-nine sat down to dinner, and Dr. Campbell Thomson was presented with a pair of prismatic field-glasses and a George III silver tankard on a base suitably inscribed, as a memento of the occasion and a mark of affection and appreciation.

THE late Dr. Henry Gervis of Bath, and formerly of London, an ex-president of the Obstetrical Society and of the Hunterian Society, has left estate of the gross value of £100,457, with net personalty of £37,233. He bequeathed to the Royal College of Physicians of London the miniature of Dr. William Hunter on ivory by Cosway, and to the St. Thomas's Hospital Medical School the engraving by Simon of the portrait by Kneller of Sir Samuel Garth, a former physician of St. Thomas's Hospital. The Royal Hospital for Incurables, Putney, receives £100, and the Bath Nurses' Home £50.

AT the meeting of the West Kent Medico-Chirurgical Society to be held at the Miller General Hospital, Greenwich Road, S.E., to-day (Friday, December 12th), at 8.45 p.m., the Purvis Oration will be given by Mr. G. Bellingham Smith, F.R.C.S., on the importance of the ante-natal clinic. This will be followed by a demonstration of the super-microscope by Mr. F. Davidson, F.R.M.S., illustrated by lantern slides. Afterwards a conversazione will be held.

DR. J. DUVERGEY has been nominated professor of diseases of the genito-urinary organs at the Bordeaux Faculty of Medicine.

A NEW in-patient block has been opened in connexion with the Church Missionary Society Hospital in Yunnanfu, China. Accommodation is provided for fifty patients, and the building includes a maternity department, an operation theatre, and a second *x*-ray plant, in addition to the one already in the out-patient department. A training class for nurses has been started, and one for midwives is planned.

A POST-GRADUATE course on diseases of the nervous system will be held at the National Hospital for the Paralysed and Epileptic, Queen Square, W.C.1, from February 2nd to March 27th, 1925. The fee for the general course, which will consist of clinical lectures and demonstrations, teaching in the out-patient department and pathological lectures and demonstrations, will be £5 5s.

THE house and library of the Royal Society of Medicine will be closed from the evening of Tuesday, December 23rd, until Monday morning, December 29th.

SIX Christmas lectures for young people will be given at the Royal Institution from December 27th to January 8th, at 3 p.m. Mr. Frank Balfour Browne, M.A., lecturer in zoology at the University of Cambridge, will deal with the habits of insects, and in the concluding lecture indicate the value of entomology to humanity, with special reference to malaria and other diseases. The programme of lectures at the Institution before Easter includes evening discourses (at 9 o'clock) by Professor T. H. Pear of Manchester on "Acquiring muscular skill" (February 20th), and by Professor Sir Ernest Rutherford on "Studies of atomic nuclei" (March 27th). Dr. Leonard Hill will give two lectures on the biological action of light on the afternoons of March 12th and 19th.

THE Fellowship of Medicine programme of special courses for January contains a two weeks' intensive course from January 12th to 24th at the London Temperance Hospital. This course will cover the whole day, and will include all the general and special departments of the hospital, in addition to a special lecture at 4.30 p.m. to which members of the medical profession are invited. Bethlem Royal Hospital, the Royal Westminster Ophthalmic Hospital, and the North-Western Fever Hospital will co-operate with the London Temperance Hospital. An intensive course will also be held in cardiology, at the National Hospital for Diseases of the Heart, from January 12th to 23rd; a series of lecture demonstrations on the treatment of common nervous diseases will be given at the West End Hospital for Nervous Diseases from January 5th to 31st. Other plans include a two weeks' course in ophthalmology at the Royal Eye Hospital from January 5th to 17th, comprising clinical work followed by lectures on the more important diseases of the eye; eight lecture demonstrations in psychological medicine at the Bethlem Royal Hospital; and a four weeks' course in urology at St. Peter's Hospital from January 12th to February 7th. The syllabus of each of the above-mentioned courses, together with the list of special courses arranged up to June, 1925, may be obtained on application to the Secretary to the Fellowship of Medicine, 1, Wimpole Street, W.1.

AN intensive course of instruction in mental disorders and mental deficiency was recently held at the Maudsley Hospital, lasting one week. It was organized by the Central Association for Mental Welfare, and was attended by over fifty social workers from all parts of the country, including the county secretaries of the Central Association for Mental Welfare, representatives of the Board of Control, lay officials in charge of institutions, representatives of county councils, of Poor Law guardians, and of the Charity Organization Society, and the almoners of several London hospitals. Lectures were given on the nature and institutional treatment of mental disorders, the methods of procedure in cases of mental deficiency, and the duties of the social worker in these connexions.

THE sixteenth international post-graduate course at Vienna, from February 9th to 21st, 1925, will deal with the diseases of the digestive organs, with special reference to treatment, and will be followed by clinical work from February 23rd to March 7th; this will include endocrine diseases and pathological conditions of the heart, blood, and lungs. The seventeenth international course on modern forms of treatment will be held from June 15th to 27th. Further particulars about these courses can be obtained from Professor Dr. Richard Wasicky, Büro der Wiener Ärztekurse, Schlösselgasse 22, Wien (VIII).

AT the next meeting of the Society for the Study of Inebriety in the Rooms of the Medical Society of London, 11, Chandos Street, Cavendish Square, W., on Tuesday, January 13th, 1925, at 4 p.m., Sir James Purves-Stewart, K.C.M.G., C.B., M.D., will open a discussion on "Drunkenness: Its Tests and Medico-Legal Aspects." Each member and associate is at liberty to introduce visitors.

AT a ceremony to be held in the Town Hall, Leeds, on Tuesday next, December 16th, during the University jubilee celebrations, the honorary degree of Doctor of Laws will be conferred upon Sir Berkeley Moynihan, Bt., Professor of Surgery, and that of Doctor of Science upon Emeritus Professor John E. Eddison, formerly Professor of Medicine.

THE late Mrs. Hulke, widow of Mr. John W. Hulke, an ex-president of the Royal College of Surgeons of England, left unsettled property of the value of £18,922. After providing for certain bequests she has given the residue of her property to the governors of Middlesex Hospital, for their convalescent home at Clacton-on-Sea, on condition that it be called the Hulke Endowed Convalescent Home. Otherwise, the residue is to go towards founding and maintaining a home for incurables.

Letters, Notes, and Answers.

ORIGINAL ARTICLES and LETTERS *forwarded for publication are understood to be offered to the* BRITISH MEDICAL JOURNAL *alone unless the contrary be stated.* Authors desiring reprints of their articles published in the BRITISH MEDICAL JOURNAL are requested to communicate with the Financial Secretary and Business Manager, 429, Strand, W.C.2, on receipt of proof.

CORRESPONDENTS who wish notice to be taken of their communications should authenticate them with their names—not necessarily for publication.

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Communications intended for the current issue should be posted so as to arrive by the first post on Monday or at latest be received not later than Tuesday morning.

THE telephone number of the BRITISH MEDICAL ASSOCIATION and BRITISH MEDICAL JOURNAL is Gerrard 2630 (Internal Exchange). The telegraphic addresses are:

EDITOR of the BRITISH MEDICAL JOURNAL, *Aitiology Westrand, London.*

FINANCIAL SECRETARY AND BUSINESS MANAGER (Advertisements, etc.), *Articulate Westrand, London.*

MEDICAL SECRETARY, *Mediocra Westrand, London.*

The address of the Irish Office of the British Medical Association is 16, South Frederick Street, Dublin (telegrams: *Bacillus, Dublin*; telephone: 4737, Dublin), and of the Scottish Office, 6, Rutland Square, Edinburgh (telegrams: *Associate, Edinburgh;* telephone: 4361, Central).

QUERIES AND ANSWERS.

INCOME TAX.

Late Assessments: Travelling Expenses.

"JORE'S" 1922-23 return was sent in late, the delay not being entirely on his part. Is it in order to make an additional assessment now for that year? His work is away from his home, to which he returns each week; can he claim an allowance for the expense of travelling?

*** The revenue authorities can, at the present time, raise first or additional assessments for 1920-21 and subsequent years. Travelling expenses are allowable if incurred wholly, exclusively, and necessarily in the performance of the duties of the office, but not where they are incurred apart from and preliminary to their performance. If, for instance, "Jore" has an appointment at N which necessitates his travelling in and around that town, then those expenses are allowable, but not the additional expenses which he incurs in travelling because he has a residence in A and travels for personal reasons between N and A.

Locumtenent's Expenses.

"PERPLEXED" states that his total earnings were £412 as locumtenent and assistant, out of which he paid £15 15s. as agents' fees, £18 as interest on loan, and £46 13s. 4d. as a life insurance premium. What should the assessment be? Is a fee of £20 for membership in the R.A.F. Reserve assessable?

*** In our view the income should be divided between locumtenent earnings, which are assessable under Schedule D, and earnings from the assistantship, which fall for assessment under Schedule E; the point is not important, except for the fact that the agency fees are deductable from miscellaneous professional receipts, but not from the earnings of "employment." Assuming that on this basis, however, no portion of the agency fees is deductable, the liability would work out as follows:

Gross earnings				£412
Deduct 10 per cent. allowance for "earned," income ...				41
Assessable income				£371
Less personal allowance				225
				£146
Of this £18 is chargeable at 4s. 6d., duty		£4	1s.	
And £128 " 2s. 3d., "		£14	8s.	
Total duty				£18 9 0
Life assurance allowance £46 13s. 4d. at 2s. 3d. ...				5 2 9
Net duty payable				£13 6 3

The reason why £18 is chargeable at 4s. 6d. is that from that amount—that is, the loan interest paid—"Perplexed" is entitled to deduct or retain tax at 4s. 6d. in the £; in other words, he need not pay £18 actually to the lender—all he is required by law to do is to pay £18 less £4 1s. = £13 19s., so that in the end, out of the £13 6s. 3d., he bears himself only £9 5s. 3d. With regard to the R.A.F. retaining fee, we understand that the amount received is assessable less an allowance for uniform, which is usually regarded as £7 10s.

Motor Car Replacement.

"A. B." refers to a reply in our issue of November 22nd (p. 982) in which it was stated that the deduction due is the excess of the cost of a new car over the amount received for the sale of the car replaced, and refers to the fact that in his district the amount allowed is the excess of the cost of the car replaced over the amount received for it.

*** The latter basis is correct if the car purchased is superior in grade and cost to the car replaced; in such a case part of the total cost of the transaction is capital expenditure incurred in improving the equipment of the practice and must be restricted according to the cost of the old car. In other cases we are of opinion that the net actual cost incurred is the legal basis of the allowance. The fee charged by an accountant for examining a practitioner's accounts and preparing his income tax return is an allowable expense.

Receipt of Rent for Rooms.

"L. A." is in general practice and rents his house at £150 per annum, subletting the top flat of four rooms for £130, inclusive of rates. How should this transaction be dealt with for income tax purposes?

*** There is no liability to assessment in respect of profits derived from letting unfurnished rooms, the reason being that any assessment in respect of such rents would fall to be made under Schedule A, and the existing assessment on the £150 rent must be regarded as covering all such liability arising in respect of those premises. On the other hand, we are of opinion that "L. A." is not entitled to ignore the subletting, because it affects the amount deductable by him as expended for professional purposes—for instance, treating the amount of local rates as £75, the amount deductable would be calculated as follows:

Total cost of premises—		
Rent	£150	
Rates	£75	
		£225
Deduct rent and rates received		£130
Net cost of premises		£95

Amount deductable on the basis that the proportion applicable to professional purposes is 50 per cent. of the unlet portion of the premises is £47 10s. There appear to be no decided cases on this point, but the above is, we believe, legally correct and also not unreasonable.

KELOID VACCINATION SCARS.

DR. W. ROSSELL JUDD (Ashton-under-Lyne) writes: In reply to Dr. J. Crisp's inquiry (December 6th, p. 1086), I have found fibrolysin injections, according to Dr. Mendel of Essen, very successful in cases of ugly scars and keloid. A young lady who was badly cut by the smashing of the screen in a motor accident consulted me for an ugly scar involving the whole of the left cheek and the anterior portion of the cornea of the left eye. Fibrolysin injections caused a marked disappearance of the scar, which is now only noticeable as a white line when the face becomes flushed. Still more interesting was the corneal scar, which caused a traumatic diplopia, and this also disappeared.

LETTERS, NOTES, ETC.

CANCER OF THE SCROTUM.

DR. WILLIAM BRAMWELL (Liverpool) writes with reference to Dr. Archibald Leitch's paper on mule-spinners' and chimney-sweeps' cancer (BRITISH MEDICAL JOURNAL, November 22nd, p. 941) to call attention to the author's question why the scrotum is exclusively affected, while the thighs and arms escape. This, Dr. Bramwell suggests, may be due to the failure of the workman to wash the part frequently and regularly. Sweeps and workers in mineral oils should, in consequence, be instructed in the importance of frequent bathing as a preventive of cancer and warned against the danger of scratching with work-soiled fingers. Such precautions, he judges, would help considerably to eliminate cancer of the scrotum.

SEA-SICKNESS.

MR. A. P. BERTWISTLE, F.R.C.S.Ed. (Appleby), writes: I have read Mr. Moxey's article in the JOURNAL of December 6th (p. 1040) with keen interest. He has certainly solved the problem of administering an unpleasant drug to a patient suffering from sea-sickness. I should be glad to hear his explanation of the mode of action of the following mixture: Sodii bicarb. gr. xv, tinct. gentian ♏ xx, sp. chlorof. ♏ x, aq. ad ℥ss. One tablespoonful is taken every four hours, or oftener. Its use is followed by very beneficial, and sometimes even dramatic, results. The advantages are: (*a*) It is tolerated when the patient is actually sick. (*b*) If vomited it serves to wash out the stomach, and so confers a benefit and is not so disagreeable as is the vomit after the administration of some drugs—for example, chloretone. (*c*) It is quite harmless, which can hardly be said of chloral if pushed to the point of somnolence.

UMBILICAL INFLAMMATION AND INFANTILE WASTING.

MAJOR J. W. LANE, M.D., R.A.M.C. (Malta), writes: A few of my patients have steadily lost weight after birth, although they slept well, ate well, had no digestive disturbance, and were not fretful. The only abnormality detected was an unhealed navel. Tying off, or rubbing off, this friable granulation-like lump resulted within three days in a steady and increasing gain in weight. This small operation was performed on patients aged from 3½ weeks to 3 months. I have hitherto observed no mention of this condition as a cause of loss of weight in young infants. Inanition and marasmus are said to occur almost entirely in artificially fed children: I suggest that this infection of the navel and not the artificial food may be the fundamental cause. I should be glad to hear of other experiences of this kind, and recommend close attention to the umbilical region in such cases. After removing the growth spirit should be applied to the stump.

LISTER'S VIEW OF SUPPURATION.

DR. JOHN W. WEIR (East London, South Africa) writes: I have read with great interest Sir Hector Cameron's oration at the unveiling of the statue to Lord Lister in Glasgow. I have no wish to criticize, but if the BRITISH MEDICAL JOURNAL of September 27th gives a verbally correct account of it there is one statement in it which, with every respect to Sir Hector Cameron, I think requires a little explanation for the benefit of those who may not be thoroughly conversant with the antiseptic theory and practice of Lord Lister. In the course of his excellent speech Sir Hector Cameron referred to a letter he had received from Dr. Collins Warren of Boston, who attended a meeting one evening of the Pathological Society of London, where he heard of a surgeon in Glasgow who claimed to be able to prevent the occurrence of suppuration in wounds, but whose claims were received with much incredulity and no little sneering. I was a student in Professor Lister's class when he introduced the new system of antiseptic surgery and was dresser under him in the wards of the Glasgow Royal Infirmary. Subsequently, when resident in the infirmary, I had for some months Sir Hector Cameron as my esteemed chief, so that I came to know of the subject at first hand. If there was one point more than another which Professor Lister wished to impress on the minds of his students it was the distinction between suppuration and decomposition. Suppuration was an alteration of the normal tissues and physiological processes which might continue in diminishing degree throughout the healing process. It was not this he claimed to prevent, but the morbid process of decomposition caused by germs discovered by Pasteur, towards the exclusion and destruction of which his efforts were successfully directed. My regard for a great surgeon and fascinating teacher is my excuse for making mention of this.

ASSISTANTS IN GENERAL PRACTICE.

"NOTON" writes: It has been interesting and amusing to read the letters on this subject in your recent issues, as I have employed many assistants and been one myself. My experience has been that many assistants are more interested in their "outings" and their time off than in their work. I am of opinion that an indoor assistant receiving £300 per annum is a very well paid man. In colliery, iron and steel works, and similar practices where accidents have to be attended night and day, and a large number of confinement cases as well, more than £300 a year may be necessary for adequate remuneration. Assistants' salaries have increased out of all proportion to private practice fees during the last twenty years. There is to my mind a great difference between hospital work and general practice; in the former one learns one's job, in the latter one employs one's knowledge to earn one's living. When assistants talk of "bonds" being objectionable, and of obtaining partnerships without capital, they are not talking business but philanthropy. In a business firm, to be given a partnership without capital implies that one is a very exceptional and valuable man indeed. Two years' premium for the purchase of a partnership, or of an established medical practice, is an extremely moderate charge—compare for example, legal practices.

"BACKWATERS OF LETHE."

MR. J. JACKSON CLARKE, F.R.C.S., writes: When Dr. G. A. H. Barton was accidentally killed early in the present year he was in the first rank of London anaesthetists. His *Backwaters of Lethe*, published in 1920 by H. K. Lewis and Co., is a short, bright, and clear account of his experience and practice in the use of anaesthetics for ordinary and special operations. The book is a safe and stimulating guide for any general practitioner who desires to become a skilled anaesthetist while still engaged in general practice, as Barton was to the end of his tragically shortened career.

A DISCLAIMER.

DR. V. S. HODSON (London, W.1) writes: It has been brought to my notice that in a recently published book the author, in addition to personal references, has added my professional address in a footnote. I wish to state that I had no knowledge that such information would be given, nor had I any idea that reference would be made to me in any form.

VACANCIES.

NOTIFICATIONS of offices vacant in universities, medical colleges, and of vacant resident and other appointments at hospitals, will be found at pages 33, 34, 35, 38, and 39 of our advertisement columns, and advertisements as to partnerships, assistantships, and locumtenencies at pages 36 and 37.

A short summary of vacant posts notified in the advertisement columns appears in the *Supplement* at page 223.